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Edward Bradford Titchener.

Studies in Psychology

Contributed by

COLLEAGUES AND FORMER STUDENTS

OF

EDWARD BRADFORD TITCHENER

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"A man keenly interested in mind, with no purpose beyond mind; a man enamored of introspection; a man to whom the most fascinating thing in the universe is the human consciousness; a man to whom successful analysis of an unresolved mental complex is as the discovery of a new genus to the zoölogist or a new river to the explorer; a man who lives in direct companionship with his mental processes as the naturalist lives with the creatures that are ordinarily shunned or ignored; a man to whom the facts and laws of mind are . . . the most real things that the world can show."

To

EDWARD BRADFORD TITCHENER

Author, Editor, and Man of Science

In commemoration of the completion of twenty-five years of distinguished service to Psychology

This volume is dedicated by colleagues and former students as a token of admiration and esteem

TABLE OF CONTENTS

A LETTER TO DR. TITCHENER

From E. C. Sanford, President of Clark College

Dear Titchener:

It is reported that in England, as in other countries, the cinema has penetrated to the remotest villages, and that in these villages—also as in other countries—there are children who are too poor to command the price of admission however small it may be made. For such children the proprietors have arranged an admission on the basis of so and so many old bottles. My own present situation is something like that of these village children. I want to have a part in this festal publication in your honor and I have no coin of the scientific realm. I have therefore hunted out three scientific old bottles which I now offer to the editors and to yourself as excuse for my presence at your *Fest*. The bottles are pretty much empty as you will see, but I am poor and they are all I have!

Bottle I. *Throwing Balls at a Target.* In the winter of 1899-1900 Prof. A. W. Trettien of Toledo University, then a student in the Clark laboratory, proposed as a topic for experimental study the question whether the satisfaction attending success in any activity exercised a favorable influence upon subsequent efforts of the same sort—in other words whether the pleasurable emotion attending success favored further success. Without raising the question of the possibility of disentangling the influence of the pleasurable emotion from the general psychophysical complex of success, experiments were begun to determine whether success in general favored further success. The activity chosen for the test was that of throwing balls at a target and experiments were carried on, with some interruptions, from Dec. 5, 1899 to May 12, 1900, and to the number of about 7,500 throws for each of two observers. The hypothesis underlying the experiment was that if success were directly effective it ought to make the average score following a good throw better than the average score preceding such a throw. The scores of both

experimenters, worked out with reference to bull's-eye throws (and the throwing of a bull's-eye was to both a distinctly pleasurable accomplishment), show, however, but small and variable differences. The same is true with reference to the scores for throws preceding and following very poor throws. So far then as these experiments show anything, they show that, under the conditions and with the experimenters in question, a good throw or a poor throw is without appreciable effect upon the success of the next following throw.

Such a result seems in some measure to run counter to the familiar experience that skill increases with practice and that one seems to progress by repeating his successes and eliminating his errors. It is possible that greater differences might appear with less practiced experimenters (the records used for study of this point were made after each experimenter had thrown upwards of 3,000 balls); or it may be that the influence exists but is too slight to be demonstrated by so crude a method.

An incidental result of a certain interest is the demonstration that in an activity which gets so much incidental practice in boyhood as throwing at a mark a very decided improvement is still possible for mature subjects. At the time of the experiment *T* was between 32 and 33 years old and *S* between 41 and 42. In the preliminary series of experiments covering about half the total number of throws the number of bull's-eyes thrown was as follows:

Experimenter *T*:

50 Bull's-eyes in 620 throws	8.06%
100 Bull's-eyes in 900 throws	11.11%
120 Bull's-eyes in 900 throws	13.33%
92 Bull's-eyes in 900 throws	10.22%

Experimenter *S*:

61 Bull's-eyes in 580 throws	10.51%
103 Bull's-eyes in 900 throws	11.44%
120 Bull's-eyes in 900 throws	13.33%
118 Bull's-eyes in 900 throws	13.11%

In later series with heavier balls and an improved target the bull's eyes are not quite so frequent and the progress, at least for *T*, is less marked. *S* seems, however, to be still improving.

Experimenter *T*:

89 Bull's-eyes in	1000 throws	8.9%
27 Bull's-eyes in	450 throws	6.0%
34 Bull's-eyes in	400 throws	8.5%
50 Bull's-eyes in	500 throws	10.0%

Experimenter *S*:

99 Bull's-eyes in	1000 throws	9.9%
45 Bull's-eyes in	450 throws	10.0%
56 Bull's-eyes in	400 throws	14.0%
48 Bull's-eyes in	500 throws	9.6%

Bottle II. *An Empirical Test of the Probability Curve.* Some years later than the throwing experiments I made, in partnership with Prof. E. C. Rowe, of the State Normal School at Mount Pleasant, Mich., then a student at Clark, an empirical test of the law of chance in two slightly different cases.

The first case was a simple matter of penny tossings, though carried out with apparatus which rendered easy the accumulation of a considerable number of observations. The apparatus consisted of a shallow tray in the bottom of which had been bored 100 shallow holes in ten rows of ten each. In use 100 pennies were shaken up in a box and dumped into the tray. The tray was then shaken sidewise until practically all the pennies had fallen into place in the holes, when the few remaining ones were quickly shoved into place with the finger. (This is of course a technical blemish but probably without actual effect, as the pennies were pushed into the holes quickly without regard to whether they showed heads or tails and without regard to the row into which they were brought.) When all the pennies were in the holes the number of heads in each row of ten could easily be counted and a record made.

In this way the number of heads in 1,000 rows of 10 pennies each was recorded. Later a further set of 24 rows was added to facilitate comparison with the theoretical distribution of 1,024 cases. The following table shows both the empirical and the theoretical results.

Heads:	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	
Empirical:												
First 1000:	2	7	43	104	204	251	221	113	49	5	1	=1000
Extra 24:			2	3	2	7	4	3	3			= 24
	2	7	45	107	206	258	225	116	52	5	1	=1024
Calculated:	1	10	45	120	210	252	210	120	45	10	1	=1024

In the second case a similar test was made with wooden button-molds of about the size of a penny. These button-molds, as everybody knows, are plano-convex in shape and should tend therefore to fall most frequently toward their convex side. They should show accordingly in the rows of ten a considerable deficiency of round-side-up cases. And so they do, but less than they should, because in the shaking of the tray the molds which have fallen round-side-up (flat side down and so more stable) tend to get under the edge of those that have fallen round-side-down and turn them over. When the experimenter noticed this tendency he tried to correct it by shaking the tray more gently but was never wholly successful in eliminating it. The influence of the asymmetry of the molds, partly counteracted by the tendency just mentioned, is shown in the first of the following distributions, which shows the number of round-side-down molds in the first 100 rows of ten. The improvement in the experimenters method (and therefore the asymmetry of the molds acting more nearly alone) is shown in the second distribution which was given by the last 100 rows of the total 500 recorded.

Number of round-side down cases in rows of ten

	0	1	2	3	4	5	6	7	8	9	10
First 100:	2	5	9	16	23	23	13	6	2	1	..
Last 100:	..	4	18	25	24	14	10	3	2	..	..

Bottle III. *The Rôle of the Intention-to-Remember-and-Reproduce.* The value of distributed repetitions in memorizing has long been established. It has even been suggested that one repetition a day might show a maximum of economy. This may very well be the case when the repetitions are made with the purpose (*Aufgabe*) of memorizing, but it is certainly not so when this purpose is absent. A very large number of single repetitions at twenty-four hour intervals may then have little or no mnemonic result.

It has been my custom for many years to read in my family the form of Morning Prayer provided by the Episcopal Church. The reading has been interrupted at various periods but at a conservative estimate I have read this form of words

at least 5,000 times in the last 25 years, usually at 24 hour intervals, often for many weeks in succession, and I am able to read it with a minimum of attention—almost automatically—and yet my memory of it is notably defective, as becomes only too patent when I lose my place in reading or fumble the turning of a page.

In order to obtain a more precise index of my deficiency I have recently made a test of how much I was able to write from memory starting myself by sight of a single word and writing as much as I could before being stopped by inability to recall, then uncovering the text until one new word was revealed and again writing as much as I could.

The first of the five prayers composing the group is the Lord's Prayer which I learned as a child and which I was able to write correctly (71 words) except for the omission of an 'and.' The group closes with a benediction (25 words) which some years ago I committed to memory intentionally. This I was able to reproduce without error.

The test with the four intermediate prayers gave the following results:

		Average number of words recollected
I.	124 words 44 promptings............	3.0—
II.	73 words 20 promptings............	3.6+
III.	146 words 38 promptings............	3.8+
IV.	158 words 27 promptings............	5.8+

The increasing size of the groups of words recalled probably indicates increasing adaptation to the method of testing, and introspection confirms this interpretation; but that the material was very far short of complete recall is abundantly witnessed by the fact that even in the most favorable case the average word group was less than 6 words and that many outright blunders—usually the substitution of words of similar general import—were made.

Such a result as this emphasizes two things: First, the dominating importance of the mental set or attitude (the *Aufgabe*) which is now generally recognized; and second, the probability that repetition under the domination of a particular *Aufgabe* tends toward the formation of habits which are increasingly specialized. This also is not unknown. As long

ago as his original experiments on the learning of non-sense syllables Ebbinghaus noticed that increasing the number of repetitions strengthened the bonds of contiguous syllables relatively more than the bonds of more widely separated members of the series. This tendency toward specialization in habits which are solidifying, if it is generally true, casts an important cross light on the matter of "transfer." If habitual activities become more specific as they are more practiced it would seem probable that the skill which such habits represent would become correspondingly less and less transferable as practice increases—in other words, that a maximal degree of possible "transfer" would attend the early stages of habit formation when the habits themselves are as yet not highly specialized. In the limiting case this would mean theoretically that a maximum of possible "transfer" would be present at the first execution of any task. Practically it would probably mean that the possibility of "transfer" would be greatest a little after the first execution when there exists something definite to transfer, but when habit is still but slightly fixed. Experiment alone can determine whether such an inverse relation between habit and "transfer" is a mere plausibility or has some basis in fact.

These, my dear Colleague, are my old bottles! If your kindly eye can discover in them any value I shall be more than happy. For me they serve their full purpose in permitting me to join with others in thus publicly congratulating you on your twenty-five fruitful years at Cornell and in expressing the earnest hope that you may have many more of like productiveness ahead. As your years increase may the sensation of their weight rise only with the logarithm of the stimulus, and your satisfactions mount in geometric ratio.

Yours faithfully,

Edmund C. Sanford.

THE SOCIAL PSYCHOLOGY OF MAN AND THE LOWER ANIMALS

By Margaret Floy Washburn,
Vassar College

Social psychology has for the most part been written in terms of behavior. It is the purpose of this paper to point out that the profound and striking differences which exist between the social psychology of man and that of the lower animals as a class are due to the presence in man of a factor which can be most concisely described in introspective terms. This factor I have called, for brevity of reference, *ejective consciousness:* it is the awareness of thoughts and feelings as belonging to other minds than our own, the awareness of the contents of other people's minds. I shall not here attempt an introspective analysis of ejective consciousness. In a paper[1] published fourteen years ago I discussed the possibility of such an analysis, but the contents of that paper need not be resurrected now either for reaffirmation or for repudiation. Ejective consciousness has doubtless like all consciousness a motor or behavioristic basis. When we think of a thought as belonging to another person, we have probably present a motor attitude unlike that which exists when we think of the same thought as belonging to ourselves. But the difference between the two states of mind, "I think or feel so and so," and "He thinks or feels so and so," is discoverable by introspection; and by noting its presence and absence, its more and less developed forms as a conscious experience, we can get much light on the modifications of social behavior.

First of all, it is clear that social behavior precedes ejective consciousness in the course of evolution. Animals are capable of acting socially, that is, of making specialized responses to other animals as stimuli, at a much lower stage of development than sees the dawn of the ability to realize consciously an-

[1] *American Journal of Psychology,* XIV, 1903, 337-342.

other's state of mind. We often observe parental behavior in the lower animals which we cannot interpret as being accompanied by any conscious realization of the feelings of the offspring. Ejective consciousness in its developed form demands the power of imagination: it involves reference to stimuli that are not capable of present action on the sense-organs. Granting then, that the capacity for social behavior exists before ejective consciousness is developed, and that the lower animals have a social psychology which can be described in terms of behavior without introducing this factor which is most immediately known through introspection, let us proceed to the discussion of the three questions with which this paper will immediately concern itself. First, which forms of social behavior have contributed most to the rise of ejective consciousness? Secondly, what has been the effect of ejective consciousness on social behavior? Thirdly, what features have marked the development of ejective consciousness itself? These are large questions, and our answers will be as briefly stated as possible.

First, since it is only on the basis of our own inner experience that we can interpret the inner experience of others, it becomes clear that the greatest contribution to the development of ejective consciousness will come from those forms of social behavior where individuals perform like functions. We shall not expect that either sex behavior or parental behavior will contribute greatly to the development of the power to interpret other minds. They may, especially the latter, contribute greatly to altruism, but altruism, as life teaches us to our cost, is not sympathy. No type of conduct in which we act towards our fellows as they are not expected to act towards us will develop understanding of their minds. In parenthood and in sex relations the individuals concerned have different functions, and cannot be expected to understand each other. Such types of social behavior as involve equality and similarity of conduct among the individuals of a group are obviously the types in which sympathetic understanding, ejective consciousness, will earliest develop. Thus the gregarious instinct and the instinct (for I would still so term it despite McDougall and Thorndike) of imitation are the forms of

social behavior which probably contribute most to the rise of ejective consciousness.

Secondly, what is the effect of ejective consciousness, as it develops, upon the social behavior which formed the pre-existent soil for its growth? The effect may be summed up, I think, in one sentence: ejective consciousness substitutes mental for physical causes of social behavior. Let us consider various types of social behavior and test the truth of this statement. The gregarious instinct has in the lower animal forms various external stimuli. In Protozoa apparently the carbon dioxide which the animals excrete is responsible for their gathering into aggregations; Small,[2] observing the instinct of baby rats to pile themselves into heaps, suggested that the desire for warmth may be the root of the social instinct; the comfortable smell of one's fellow-beings, the safe and accustomed sights and sounds due to their presence, incite the grouping of higher animals. Man, on the other hand, with better developed ejective consciousness, can find little comfort in the society of persons whose mental processes he interprets as wholly unlike his own. The society of an imbecile would be worse than solitude. The self-exhibiting instinct is stimulated in man rather by what he imagines other people are thinking of him than by any external stimuli. It becomes a strong ally of the gregarious instinct: whether the individual concerned is a woman going to an afternoon reception or a man seeking conversation at a corner grocery, the desire to see oneself favorably reflected in the minds of others, whether on account of one's clothes or one's arguments, is a powerful incentive. The imitative instinct becomes so altered by ejective consciousness that its chief stimulus is not the outward actions of others, but what we conceive to be their mental states, their "beliefs and desires," as Tarde put it. The instincts of sex and parenthood, as befits those which have done least for the development of ejective consciousness, are least modified by it. External stimuli are still powerful in both of them: the influence of the physical contact of the child in one's arms is evident in those women who, like the lower animals,

[2] Willard S. Small, Notes on the Psychic Development of the Young White Rat. *American Journal of Psychology*, XI, 1899, 80ff.

love their children best while they are very small. But even in sex and parenthood the mental stimuli assert their claim in the case of human beings: the lover persuades himself that his attitude is at least partly due to the mental and moral qualities which his ejective consciousness ascribes to the object of his affections, and the mother supplies herself with a mental stimulus for her instinct by thinking her child a model in intellect and character.

The chief effect upon social behavior of this substitution of internal or mental stimuli in place of external or physical stimuli is to render it *steadier* and *more constant*. This effect is produced in two ways. In the first place, the operation of social instincts is rendered independent of outside conditions, so that a human being may act socially in the absence of the person to whom his action relates. Animals can form only physical crowds, and must as a rule have directly before them the animals they imitate. Human beings, affected by their ejective reading of the contents of other minds, can form crowds with persons far removed from their sight and hearing, and imitate them steadily and consistently whether they are present or absent. The self-exhibiting instinct, originally called forth only in the actual presence of the female or a hostile male, when its stimulus becomes the thought of what other people may think of us, is transformed into a steady, guiding incentive that in persons with highly developed ejective consciousness may be relied upon to keep behavior social even through long periods of solitude. The fighting instinct, when its stimulus is not the presence of the enemy but one's imagination of his thoughts and feelings, gives rise to attitudes of steady and unremitting hostility instead of to sharp and brief conflicts with no "bad blood" left behind. The parental attitude in animals does not long continue after the offspring are removed; in human beings it lasts through years of absence. The faithfulness of an animal to its mate is probably due to the fact that other animals, by reason of their slight physical differences from the mate, fail to supply an adequate physical stimulus for the mating instinct, which has become so modified and specialized that not the generic physical marks of all females, but these *plus* the special physical marks of a par-

ticular female, are its proper stimulus. The faithfulness of a human being to his mate rests on the permanence not of physical traits but of the features of character and mind which he ascribes to her, and may continue long after the physical traits have altered.

In the second place, the operation of social instincts is set free, comparatively speaking, from the dominance of changing physiological states. An animal's instinctive behavior has always two sources, the physical force or stimulus, and the inner condition of the animal. Unless the physiological condition is favorable, as at the mating season, the presence of an individual of the opposite sex fails entirely to call forth the mating reaction. When a certain time has elapsed since the birth of offspring, the protecting attitude of the mother turns to hostility: her physiological cycle has passed on to a new phase. Herrick has pointed out how in birds the instincts of mating, nest building, incubation, and feeding the young follow each other in an order which depends not on the succession of outside stimuli, but on the succession of inner physiological states. In human beings, the freeing of the social emotions and of social behavior from the dominance of physiological states is the greatest practical problem of the emotional life. It is only imperfectly accomplished. We human beings have constantly, in order to avoid wounding the feelings of those we care for or seeming disloyal to the causes we have adopted, the task of feigning in times of unfavorable physiological state the emotions that we feel in full strength only when the physiological state is favorable. But it is precisely because we do thus call to mind the feelings and opinions of others, because we have ejective consciousness, that we are able to feign steadiness and continuity of emotion, and in feigning the emotion, to feel it in some degree at least. And occasionally we meet the truly "spiritual" personality whose enthusiasm for humanity seems always burning with a steady flame unaffected by the rise and fall of physiological states.

It may be said that these effects upon social behavior which we have ascribed to ejective consciousness, to the imagination of what other people are thinking and feeling, can equally well be ascribed to the imagination of other people's behavior;

that it is our power to represent in thought what other people will do, rather than our power to represent what they will think or feel, that is responsible for the greater steadiness and continuity in the social behavior of man, as compared with that of the lower animals. I am perfectly willing to admit that the physiological basis of ejective consciousness may be a kind of inner imitation of the behavior of others, but the inevitable accompaniment of such inner imitation is the consciousness of other people's states of mind, and this consciousness may be directly observed by introspection, whereas the movements of inner imitation cannot be directly observed in any complete degree, and have rather to be inferred on theoretical grounds.

Our last question concerns the changes through which ejective consciousness itself passes in the course of its development, both in the race and in the individual. These changes may be summed up under three heads.

First, the evolution of ejective consciousness has involved a continuous broadening of its spatial and temporal reference. The individual or the race with little developed ejective consciousness is capable of interpreting the minds only of those close at hand in space. The person thus limited has little sympathy with persons whom he does not often see; the race thus limited regards all other races as enemy aliens. The undeveloped ejective consciousness is equally limited in its time reference; it cares nothing for history and believes in letting posterity look after itself. Savage peoples and unenlightened individuals sympathize with their contemporaries only.

Secondly, as ejective consciousness evolves it becomes less emotional and more intellectual. Emotions, in our fellow-beings, produce of course more striking physical manifestations than ideas do. When ejective consciousness is but slightly developed, it is still subject to the influence of the external stimulation. The cry of anguish actually heard, the grimaces of the moving picture actor 'registering' emotion, bring realization of the state of mind they imply home to minds which would be incapable of grasping the thoughts of another person when those thoughts differed from their own. More-

over, emotional ejective consciousness is more primitive than intellectual ejective consciousness not only because it has stronger physical stimuli, but because there are only a few emotions, and there is an infinite number of ideas.

Thirdly, the development of ejective consciousness has passed from the ability to interpret processes in other minds as like those in our own mind, to the ability to conceive states in other minds as different from those in our own mind. The person of rudimentary ejective consciousness easily conceives that other people feel and think with him: he cannot bring himself to the point of realizing that they sincerely differ from him. We must of course put limits to the power even of the most enlightened in this direction. No one can imagine in another mind a state wholly unlike any that he has ever experienced. But the person of highly evolved ejective consciousness can, at a given time, set side by side in his mind and contrast his own ideas and those which he realizes exist in the mind of a fellow-being. He can say, "I think and feel thus and so, but my neighbor thinks and sincerely feels in this other quite different manner." For the individual whose ejective consciousness is little developed, if his neighbor's mind is interpreted at all, it is only as an extension of his own mind at that moment.

PRINCIPLES OF EXPLANATION IN PSYCHOLOGY

By W. B. Pillsbury
University of Michigan

Within the last few years there has been more diversity of explanation in psychology than ever before. True, none of the new suggestions are altogether new, but not often have so many diverse tendencies presented themselves at the same time or in such close succession. Most of the possible forms of explanation seem to be finding new adherents who support each his own form with greater emphasis and even bitterness than is the wont. In the past, fashions in explanation have come one at a time. While the fashions succeeded each other rapidly there was usually some one that was *the* fashion and the members of that school or cult could exert sufficient discipline to shame others to silence or could make sure that they had no hearing in really scientific circles. Just now emphasis seems to be put upon aggressive assertion, and the more paradoxical the view, the wider the acclaim with which it is hailed. While scientific brow-beating is attempted, too many and too vigorous partisans are active to make the cowing altogether effective. It seems then a proper time to attempt to determine what the accepted forms of explanation are, to see how far they have anything in common and, if not to harmonize, at least to pick out the essential features of what may be regarded as a real explanation in psychology.

For purposes of illustration we can best choose the antecedents of action, since all forms of explanation are implied in the discussion ordinarily given of it. We can then discover what methods are used in the investigation of a complex process and so reduce the principles and entities involved to the simplest and fewest possible.

Two methods of investigating the active functions must be distinguished: the observation of mental content; and a study

of the wider relations or interconnections of the observed or physically recorded phenomena. Each method has contributed its share in reaching the present conclusions, but obviously each is fitted to give results in different ways and in different degrees. Direct observation of the action or will process has led to a gradual reduction if not to the complete elimination of the will element as a mental process. First we find the assumption that there is a direct will element. This was held in the pre-observational stage when purely rational methods were used rather than observation, even by the associationists and others who claimed to make use of an empirical method. As introspection came to be used it was more and more difficult to discover any structure that could be identified with will. In Wundt we find the peculiar active consciousness emphasized and have it given concrete embodiment in the awareness of the discharge of motor impulses, the innervation sense. Münsterberg's *Willenshandlung* and James' more theoretical discussion showed that this was not well based either in psychology or physiology and we have its author gradually giving it up and replacing it by feelings of strain of kinaesthetic origin and gradually putting less emphasis upon the phenomenological and more upon the reasoned theoretical explanation of the directing forces. Feeling of activity still has a prominent part but it is as accompaniment rather than cause, a sign of underlying forces, rather than one of the actual forces themselves.

In the quarter century that has elapsed since that time, one after another of the elements that accompany or precede the movement or that may be thought to accompany or precede it has been selected as the essential factor or as one of the essential factors in the total complex. Thus in the reaction against the innervation sense which was at once cause and accompaniment, we find the assertion by James that the incentive to movement is the antecedent memory of the kinaesthetic sensations that are to come when the movement is made. To perform an act a memory of how that particular movement feels must be aroused. In the later studies this was given over in part and any idea of the movement or of the end of movement was permitted to serve as the immediate

incentive or cause of action. Titchener admits the idea of the end and the idea of the movement, Woodworth eliminates all ideas of an explicit sort and substitutes the imageless intention to move in the way desired. That an idea or some substitute for it, such as sensory stimulus, is present before each act is fairly clear. One other question remains to be raised and that is: Are these antecedents the real cause of the movements?

The most active opponent of this view is Thorndike. In his 'Ideo-motor Action'[1] he argues on the basis of the opinion of members of the Psychological Association that the idea which accompanies or precedes the action has little more effect upon that action than has the melting of a wax image of a man upon the health of the individual imaged. He tests by statistical methods the belief of a group of persons, assumed to have a basis for their belief. Granted that the interpretation put upon the returns justifies his conclusions, it gives no evidence as to the rights of the individuals in question to have an opinion or at least to have the opinion that they express. In brief Thorndike's article is a challenge to the psychologist to determine the criteria that distinguish causal relation from mere succession in psychological phenomena. He grants that the idea of movement in some form usually precedes the movement itself, but asserts that it is entirely irrelevant to that movement. Before we raise the more general theoretical question it is well to point out that Thorndike's refutation of the connection between the idea and the movement grants all that any one really claims for the ideo-motor explanation of action in that he excepts from his refutation ideas that have been connected with the act by 'heredity, use, and satisfying results.' Bair long ago showed that merely picturing a movement that had never been made by the individual had no effect in arousing the act, that one might as well try to raise a window by thinking of its going up as to try to move the ear by merely picturing its movement, before he had actually made the movement. All that one needs to prove, then, is that thought of a movement that has at some

[1] E. L. Thorndike, Ideo-Motor Action, *Psychological Review,* XX., 1913, p. 91.

time succeeded it is the real cause of that movement. Our first problem is 'what is the criterion of a causal relation in psychology?'

The problem of cause has been quite as much disputed in the physical sciences as in psychology. Physical causation is assumed where there is frequent connection (Hume), where in addition there is 'sufficient reason' for regarding the connection as more than one of chance, or in the latest form where it may be assumed that some energy actually passes between the cause and the effect. Since, however, the assumption that energy passes is more often dependent upon assumed cause than capable of direct measurement, we are thrown back upon less definite criteria. These are usually found in analogies with other connections recognized as causal, a harmony with the earlier experience of the individual as to what is and what is not a connection of a necessary rather than of an accidental nature.[2] On the more objective side we have the canons of induction. Whether these would be accepted as proof without the reinforcement of being able to fit the conclusion into a larger system of knowledge without the general belief that some energy passed, is a question. Certain it is that the causal relation will not be assumed unless the connection postulated as causal satisfies these canons. If they hold on a sufficient number of repetitions, their evidence is accepted, unless the conclusion is very much out of harmony with general principles, or with the body of organized knowledge.

Applying the canons to the antecedents of action, we find that some conscious process related to the act invariably precedes it. The conscious element varies greatly from moment to moment and from act to act. Only in movements of an average degree of familiarity is the idea definitely a memory of the exact movement. In more familiar acts the antecedent is an idea of the end to be attained and this comes to be more and more remote and consequently fainter the greater the familiarity or the more completely it is part of a larger movement com-

[2] W. B. Pillsbury, The Psychology of Causality. *Philosophical Review,* XIII., 1904, p. 409.

plex. In the end we have nothing more than a mere general intention as the antecedent of the act. Repeated introspections by Woodworth and by several of the investigators of reaction times tend to show that some idea of end or intent is in a large percentage of cases associated with the act and that the nature of the act varies with the intent. The method of differences gives the idea of the end rather less support. Now and again acts are made with little explicit idea of why they are made. Acts also come although knowledge of their purpose is lacking. In other cases the act is not in harmony with the purpose. One intends to do one thing and does another. These are exceptions to the general rule and can usually be explained by the presence of other influences.

One of the most frequent causes of the failure of a purpose is to be found in the great strength of some idea incompatible with that purpose. This is another phase of the same general principle that the determining factor is the idea in mind, but in this case it is an idea that is not included in the general purpose. Here again one can show the frequent succession between an idea or sensation and the corresponding movement. General observation shows that many cases of misspeaking one's self and slips of the pen arise when some idea becomes too prominent. Langfeld's study of the effect of directing an individual to avoid the edge as compared with asking him to keep in the middle of the groove shows experimentally the same general relation. That the idea plays a part is confirmed by the method of differences, since as the idea is changed the movement is changed, and when the right idea is lacking the movement does not result. Both of these canons indicate that general intention and the immediate idea stand to the movement as cause stands to effect in the physical world. What is also apparent is that in some measure the two influences are opposed. When the general purpose lapses or some particular idea becomes strong, the movement is determined by the idea; under ordinary circumstances, the intention dominates and the idea is either in harmony with it or is of no avail.

Even more complete when tested by the inductive canons is the evidence that the physical stimuli, grouped by James

as the resident and remote sensations, play a part in controlling the direction and accuracy of movements. Eliminate the stimuli and the accuracy of the movement is markedly decreased. The resident sensations are eliminated by disease, the organs may fail to develop in muscle or tendon, or the connections in the cord may be broken through the degeneration of the posterior tracts. The remote sensations may be disturbed more easily by closing the eye or stopping the ears. This of course brings with it loss of control in very marked degree. Whatever may hold of the conscious accompaniments the dependence of movement upon stimulation, and upon the particular forms of stimulation mentioned cannot be questioned.

To stop the analysis with the statement that stimuli of various kinds are responsible for the acts, evidently leaves one considerably short of the solution of the problem. The stimuli behind both the remote sensations and the initiating stimuli and sensations are seldom unambiguous. Many stimuli are present in almost equal strength and where the stimuli are of different strengths the weaker often controls the movement to the exclusion of the stronger. One evidently cannot reduce the control of movement to the stimuli as such. Even Watson admits this in his reduction of behavior to *controlled* reflexes. One must seek the controls. One, attention, is perhaps the characteristic most frequently mentioned. In many uses the word only designates the fact that there is control rather than stating the controlling conditions, but some advance has been made in reducing those conditions to simpler elements. The most immediate and definite of these, although frequently designated as an independent condition of action, is the influence of the attitude, purpose, or '*Aufgabe*' of the Külpe school. Ach has shown experimentally that this satisfies the inductive canons as a cause of action. The action corresponds directly to the directions given or to the attitude of the subject as a result of some antecedent experience. On the basis of general observation one may add more remote experiences to the group of events that aid in selecting one from among the many stimuli to control the act, or that control the course of thought in which the particular memory

that initiates the act constitutes a link. From stimulus we must go back to attention, but attention proves to be a name for purpose or attitude and earlier experience. Recently, Thorndike and, before him, many others have argued that the determining factor in the selection of the stimulus that shall produce movement is not attention but the pleasantness or unpleasantness that has accompanied an act in an earlier connection. This can be subjected to similar empirical tests. All this is dependent upon assuming that the same tests of causal connection may be applied to human acts as to physical processes.

As an empirically determined connection an act can be said to result when there is some idea, however vague, or whatever the imagery, provided only that the content has at some time in the past been connected with the movement. The idea may be the result of a train of associations or of some physical stimulus. Where several ideas are possible the selection may be in terms of attention or of the pleasure that accompanies or has accompanied the act connected with the idea. Attention either is determined by or is partially equivalent to the task that is set or the general mental attitude of the moment. These in essentials would be said by most psychologists to constitute the antecedents of the act. But with the enumeration of the antecedents, agreement stops. When one attempts to assert which of the antecedents is the true cause, the cause which will stand the test of agreement with other phases of experience, violent dispute begins. At the extremes we have the animists represented by McDougall and those who believe that all must reduce to purely physical connection represented by Thorndike and perhaps in fuller sense by the behaviorists, although they are still far removed from the point of attacking so complicated a problem. A comparison of the arguments is interesting rather as illustrating the difference in the premises than in giving hope of finding common ground.

For McDougall the premise seems to be that whatever is shown to be related to the interaction of many parts of an organism cannot be the product of or in any way dependent upon the organism, but must have an independent existence and an independent cause. Voluntary action is dependent

upon more than one part of the organism, is the result of the interaction of various stimuli, and hence must find its real cause in something else. That something else is pure thought or conation, a process allied to meaning. Feeling is also a similar animistically conceived entity holding a relation to the organism as a whole rather than to any separate part and must therefore be an uncaused cause. The real cause of voluntary action, as of all else above the simplest reflex, must be found in the idea as pure meaning. Thorndike asserts on the contrary that the relation between idea of any sort and the act is similar to that between the waxen image of the savage and the man it represents. For one it is the essential cause, for the other it is mere concomitant.

Our problem is to seek objective justification for one view or the other. Here advance seems absolutely barred. Both men deal with such words as 'absurd,' 'inconceivable,' 'accepted impossibility of demonstration,' etc., but when one attempts to discover objective argument it is not so clear sailing. After the vigorous assertions, Thorndike lands in the conclusion that pleasure acts to make learning possible. "A neurone modifies the intimacy of its synapses so as to keep intimate those by whose intimacy its other life processes are favored and to weaken the intimacy of those whereby its other life processes are hindered. The learning of an animal is an instinct of its neurones."[3] McDougall[4] comes out of his maze of reasoning with the conclusion that even instincts are purely psychical, that pleasure as a purely psychic somewhat acts upon conation, a similar psychic element, and that in turn in some way modifies or initiates the bodily processes which result in action. The real major premise behind the conclusion of the one seems to be that only mechanism gives a real explanation, behind the other that the real moving force in the universe, at least in the organic universe, is mind. Each seizes upon any fact that cannot be fully explained by the opposing system (and such facts are legion) as a conclusive argument in favor of his own theory.

It is evident from the foregoing discussion that all are fairly well agreed as to what are the conscious and physiological

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[3] E. L. Thorndike, *Animal Intelligence.* 1911, p. 247.

[4] W. McDougall, *Body and Mind.* 1913, Chapter XXIII.

antecedents of action, but it is equally obvious that the final decision as to which of these are essential, which are really efficacious in causing the movement, is not to be expected at present or at any time until thinkers are more nearly agreed than they promise to be for some time on general metaphysical theories. It is primarily in the second criterion of causal relationship that the disagreement arises. For one type of mind one set of facts seems to warrant the assumption of a causal relation, for the other type of mind it does not. Since one cannot hope to change these mental types by argument, since indeed their own conclusions are reached with no argument aside from the fact that the opposite seems inconceivable, the only course is to pick out the elements upon which they do agree and determine how far the remainder is essential. Eleminating disputed points we may conclude that action comes when an idea, stimulus or general intention is present that has at some time been connected with the act in question. Where one has only the general intention without definite idea, the stimulus must have been frequently connected with the movement, or the movements have been made frequently in immediate succession upon the preceding movement or movement element.

Similar agreement upon the wider determining factors may also be reached if we are content to state them objectively rather than to consider the theories developed to explain them. Attention, pleasure, the purpose or task, may all be put in the same class. The last is more empirically determined, less general, and probably to be regarded as one of the conditions of attention. Pleasure and attention are no doubt in some way closely related, as is evident from their partial identification by the system makers. Attention is either caused by pleasure, or pleasure by attention, or pleasure is an indication of the forces active in producing attention. Both may be regarded in part as the expression of the nature of the individual determined by heredity and education. The more detailed analysis may be left to experiment and observation; the final name given to the single function can be decided by the system makers. When both are fully elaborated we shall have the conditions for Watson's 'conditioned

reflex,' to be stated if he likes in more biological terms. Work of this kind can determine the wider antecedents of the voluntary act and group them in classes ready for use by the theorist, whether he expresses them psychologically or biologically. We can expect agreement upon these classifications whether it is or is not reached on the wider problems. It is quite as important to classify these functions as the simple structural elements.

As an instance of the more fundamental questions which seem to depend upon observation but really can be answered only in terms of general considerations, one may mention the final release of the act. It has been frequently asserted that the mere presence of the idea no matter how pleasurable or how completely attended to, is not inevitably followed by the correspondent movement. Thinking of sneezing does not always make one sneeze, thinking of getting up does not always result in rising. What else is needed is stated by the more mechanically minded in the phrase 'the conditions must be just right' or 'the physiological antecedents must all be present.' On the more idealistic side the same phenomenon is referred to some added mental element,—explicit consent for James, conation by McDougall, identification with the ego by Ach and Michotte. The men who accept this fact give it directly opposite interpretations. Thorndike uses it as proof of the complete inadequacy of ideas of all types and the complete dominance of the physiological. Ach and McDougall assert that it proves the absolute necessity of a pure thought element that has no dependence whatever upon the physical. The dispute is so sharp that one may well question the importance of the factor. All agree that complete dominance of an idea connected with an act in the past usually is followed by that act. The exceptions may well be ascribed to inaccuracy of observation, such as failure to recognize the presence of another opposing idea or stimulus.

One final question remains, that is the most fundamental one, whether ideas or conscious processes in general are to be given any efficacy in determining acts. On this I suppose no more can be attempted than to develop a working hypothesis that shall leave room for the most fundamental difference of

opinion. Of course we can never hope to meet the position of the behaviorists on this point. They have closed their eyes to the presence of the idea in all of its forms. For the others the only question is whether the conscious state, including the idea or pure meaning is a real cause or merely a sign of the true physiological causes. This question involves the most perplexing phases of the body-mind problem. When one attempts to apply to it the second test of cause, the harmony with other instances accepted as causal, two opposing analogies arise in approximately equal strength. On the one hand, an idea does not seem to have a place in the series of physical causes, Thorndike's classification of ideo-motor action with magic seems plausible; on the other, it is difficult to see why ideas should so universally precede acts if they have no causal function. Admitting the force of both analogies, there is still good methodological reason for continuing to discuss the action problem from both the mental and the physical point of view. First, one analogy is as strong as the other and the more radical arguments in favor of one or the other all seem to be forced and rhetorical rather than real. In the second place, one must continue to use consciousness in practice, for whether a real cause or not, it is a valuable and in many cases the sole indication of the presence of physiological forces assumed to be effective by the most mechanistically minded of psychologists or physiologists. So far as the practical methods of investigation are concerned we must proceed in the same way whether idea or other conscious process is the ultimate and only conceivable cause of all action or whether it is only an indication of purely physical processes. The upholders of opposing sides may be left to continue their argument over the fundamental question.

If we use this discussion of the problem of action as typical of the methods which must be used in psychology, it may be laid down that the first aim must be to establish the empirically determined successions between definite mental states or events, between physical and physiological processes and mental events and between mental events and the physiological and physical events. Where mental events are directly involved introspection must be used, first to describe and analyze the mental events and then to observe

the order of succession of these events and of bodily movements. The succession of events may be described and classified quite as completely as the structures. The simplest, the laws of association, were recognized before any progress had been made upon the mental elements and long before any knowledge of the physiological forces that are now supposed to account for them. The wider control processes may also be similarly grouped and the empirical laws determined. Whether the '*Aufgabe*' acts as mental force or is an expression of drainage or of facilitation and inhibition of cortical elements, the fact must be mentioned and the laws of its action stated. Similarly, whatever one's opinion as to the ultimate nature of meaning or the 'imageless' processes, they are empirical facts that must be recognized and have the laws of their action determined. Final explanation may be postponed awaiting fuller knowledge. Structures and functions may both be treated by introspection and objective experiment and the results must be valuable whatever the final system into which they are organized.

After this work has been done and also going step by step with it, we are bound to have the wider interpretations, the attempts to decide which of the empirically determined connections are causal, which are merely fortuitous. On these points one cannot expect agreement; the experiences and prejudices of the workers are too different. Fortunately this work is most certain to be done. Once the preliminaries have been got out of the way and even before preparation is complete, theorizing is sure to begin. The instinct for unification and explanation is sufficiently strong to assure some grouping. Ultimate agreement on the most fundamental questions cannot be expected but in the meantime much may be done. While waiting for the final system, theoretical controversy is better than stagnation and even contributes much of the incentive to observation and experiment as well as to the formulation of preliminary generalizations. As Titchener[5] says "Psychology must be built up by facts and logic." Introspection and observation together with logic may supply the facts; each worker is responsible for his logic.

[5] E. B. Titchener. *A Beginner's Psychology.* 1916, p. 330.

THE CONTENT OF RELIGION AND PSYCHOLOGICAL ANALYSIS

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The psychology of the twentieth century has witnessed certain changes in the analysis and evaluation of consciousness. The old psychology was infra-metaphysical in its philosophical deductions from the principle of soul. The new psychology became supra-physiological in its psychophysical treatment of the brain. With the obvious differences between such philosophical and scientific methods of psychological analysis, there was much in common with the two methods in that both aimed at system, both sought to lay down fundamental principles. From being doubly theoretical, psychology has now shown two distinct tendencies toward the practical or applied; here it is social and industrial, there it is popular and religious. It is the just aim of psychology to consider its data apart from any logical validity or ethical value, and the consideration of consciousness as such can commit no error and do no harm as long as psychology recognizes its self-imposed limitations; but when this attitude of neutrality passes from the non-logical and non-ethical to the anti-logical and anti-ethical, there is danger of psychological confusion, a condition of things not unrecognizable in the new psychology of religion. In the psychological analysis of religious consciousness, the science which once kept close to either soul or brain has now betrayed a tendency to exalt the trivial; freedom from metaphysical and moral responsibility has resulted in pseudo-scientific study of conscious states which may be curious enough, but are far from being typical or fundamental. Psychology of religion has thus become a mixture of the social and popular duly decorated with certain mental curiosities all-too-human.

To bring about an understanding between the living content of religious consciousness and psychological method, it is

necessary to overcome difficulties which arise on both sides of the house. From the viewpoint of religion, religious consciousness which seems to crave psychological analysis places itself in a dubious position when it asserts that its soul-states are by nature *inscrutable*. Thus the right of analysis is claimed and denied in the same breath. On the scientific side of the entente, it is asserted that psychology does have the right to analyze religious experiences; but when this abstract right is exercised, it appears that psychological method is guilty of *irrelevancy*. Religion seems to say to psychology, 'You may analyze my precious soul-states, but you must look upon them as beyond the scope of analysis.' On the other hand, psychology asserts, 'We have the right to analyze anything peculiar to consciousness, and we intend to employ in religion the same methods which we have used elsewhere.' Between the two stools of the inscrutable and irrelevant, the content of religion falls to the ground. If religion desires an analysis of its consciousness, it must submit to analysis; if psychology exercise its analytical rights, it must be sure that it is in possession of appropriate methods. If the Czar wishes to come into right relations with his people, he must cease to be Czar, and if the Russian people desire to adjust themselves to the State, they must give up their nihilism. Between the extremes of tyranny and anarchy, there must be something like a State; and between the opposed poles of inscrutability and irrelevancy, there should appear a just psychology of religion.

The appeal to inscrutability is unworthy of religion and unfair to psychology to which appeal is made. Religion can avail itself of psychological analysis without surrendering to psychology, since the content of religion, instead of being inscrutable and unanalyzable, is unique and characteristic. A thoroughly psychological analysis of religion need not be shown to be false to convince us of its irrelevancy. The whole religious field may be covered *in extenso* without unearthing the intensive content of that which is supposed to be analyzed. The same may be said of the aesthetical. Psychology may analyze a painting by Raphael when it shows that the effect is produced by means of drawing, coloring, perspective, and

the like; but the characteristic totality of the aesthetic impression has escaped the detailed analysis. Art as well as religion is on its guard against the aggression of psychology. "Thorough-bass and religion," said Beethoven, "are subjects which are beyond the range of explanation." In the same spirit of independence, Baudelaire asserted that "Poetry cannot upon pain of death and destruction submit to the dictates of science or ethics." There is a psychological aspect of art and religion, just as there is a psychological view-point with everything which comes into human consciousness; but from this general truth, it does not follow that the identification of conscious states is a complete description of aesthetic and religious consciousness.

There is psychology and psychology; as soon as this fact is recognized, there need be no further disagreement between psychologist and religionist. On its scientific side, psychology has been extensive and explanatory; much that is characteristic of its subject-matter has of necessity escaped it. Intensive psychology, even when it has not given the name of 'psychologist' to its practitioner, has been able to lay firm hold upon the content of its subject-matter. Among such intensive psychologists, we may note the names of Poe, Baudelaire, Stendhal, and Bourget, in the aesthetic field, Dostoievsky, Nietzsche, Ernest Hello, and J. K. Huysmans in the religious. The intensive psychologist has not seen fit to refer this data to any biological basis, but he has not failed to apprehend the content of religion in its integrity. He has overlooked the formal nature and subordinate effects of the conscious state for the sake of grasping their living content. In this spirit of intensive psychological analysis, he has come nearer to the heart of his subject than has the professional psychologist.

That which the psychologist of religion is supposed to do is to appreciate the value of the experience of the subject-matter which he attempts to analyze. Religion as such, especially in the form of the Ritschlian theology, has often sought to indicate that the religious feeling is a sense of worth expressible in the form of a value-judgment; but that is not quite the same as the general contention to the effect that

religion, like other conscious products, is itself a matter of value. The difference between the two views may be expressed as follows: according to the special formulation of the religious problem, religion consists of value; according to the general notion of religious psychology, religion is that which has value. Now, the difference between consisting of value and having value is not to be overlooked. Whether religious consciousness is identifiable in the form of a value-judgment or not should not prejudice us against the general proposition that religion as such has worth for man. And it is just that worth of religion for man that professional psychology of religion is in danger of overlooking. Various products of consciousness, aesthetical, ethical, and religious may be said themselves to have value, even when their specific content is not made up of a value-judgment. Psychology is naturally committed to the subjective; nevertheless, psychology has the right to indicate that its data, far from having existence and essence alone, are possessed of "meaning," a view-point indicated by Pillsbury.[1]

The intensive psychology of religion, which assumes that religious experience is characteristic and valuable, is removed from extreme views which hold that religion is wholly free from or wholly subordinate to psychological analysis. The religionist cannot proclaim that he is dealing with so much soul-stuff, and then insist that this material is superior to analysis; nor can the psychologist insist that, since religion has its psychological aspect, it is wholly in the hands of the psychologist. Religion may not be free from the psychological touch where it is by its nature delivered from the psychological embrace. An example of this Germanic right to invade and possess the field of religion appears in Leuba's *Psychological Study of Religion*,[2] which constantly suggests that "theology is a branch of psychological science" (p. 276). In vain does such an imperial psychology invite the religious consciousness to repose in its Procrustean bed, for it is only by a process of stretching out or cutting off that religion can be fitted into

[1] W. B. Pillsbury, *Fundamentals of Psychology*. New York, 1916. Chapter XI.

[2] James H. Leuba, *A Psychological Study of Religion*. New York, 1912.

the forms of psychological science. To take religion for what it is worth and measure it according to a psychological standard is indeed no simple task, but the example of Stratton's *Psychology of the Religious Life*[3] shows that this can be done in a manner acceptable to the fair-minded among both religionists and psychologists. Stratton's work follows an exact plan of psychology when it divides its material in the standard form of cognition, conation, affection; then, it squares accounts with religion when it lays such great emphasis upon the principle of 'conflict.' Furthermore, the genuineness of the study is shown in the careful and complete examination of religion from the standpoint of history. The result is to identify the content of religion, and to subject it to an analysis critical and appreciative.

Apart from the contrary tendencies in contemporary psychology, the analysis of religious consciousness is made difficult because of the peculiar, though not inscrutable, character of the subject-matter in question. As early as the days of Hume, when the psychological analysis of religion had hardly begun to be, it was pointed out that religion "springs not from an original instinct or primary impression of nature, such as gives rise to self-love, affection between the sexes, love of progeny, gratitude, resentment; since every instinct of this kind has been found absolutely universal in all nations and all ages. . . . *The first religious principles must be secondary*."[4] Because of the 'secondary' character of religious consciousness, psychology has generally found it possible to cover the whole range of conscious life without involving a single datum of religious feeling. A thoroughgoing treatise of psychology, such as one finds with Wundt or Titchener, feels no need of involving religious emotion in the long list of topics treated. Even such broad twin-volumed works as those of Sully and James find it unnecessary to include any extensive discussion of the religious problem, if any at all. The upshot of the whole matter seems to be that, when the psychologist makes a sincere study of his material, he leaves the data of religious consciousness out of account; and it is only

[3] G. M. Stratton, *Psychology of the Religious Life*. London, 1911.
[4] David Hume, *The Natural History of Religion*. London, 1757, (Intro.).

when he adopts the rôle of the 'psychologist of religion' that he feels free to consider the question under discussion. How does it come about, then, that religion is and is not the subject of psychological inquiry? How can James ignore the subject in his general psychology and then devote a whole volume to it in another connection? How can psychology succeed in the case of Stratton and fail in the case of Leuba?

The answer to this series of questions may perhaps be found when one asserts that religion becomes the subject of psychological investigation only as the characteristic content or meaning of religion is taken into account. If conscious 'immediacy' is sought in the same spirit that one seeks the directness of sensation or feeling, the point is lost; but if such 'immediacy' is of another kind, say, a complex of that which has worth to the subject who experiences it, the success of the psychological undertaking is assured. Such immediacy is not a bit of non-psychological inscrutability, but a view of consciousness to be apprehended and appreciated by him who will take it for what it is worth as that which has character and content. Upon this basis of significance, character, and worth, the psychology of religion can be effected. To consider religious consciousness in a purely sacerdotal way or to regard it as just so much more scientific matter is to defeat the cause of religious psychology. In recent years, the science of psychology has suffered from the attempt to make it social and popular, so that it is in the interest of both religion and psychology that one insists upon a genuine analysis of religion's characteristic content.

Viewed *eo ipso,* the religious consciousness appears in the form of want, need, dependence, or their equivalent. As Stratton has emphasized the principle of conflict, we may point out that the perpetual contrast between the immediate and remote springs from a sense of need to which also it ever returns. The genius of religious consciousness appears more clearly when religion is contrasted with art. In many ways, these two forms of culture have much in common, although there comes a place where they diverge and disagree. Both are extra-products of consciousness in that they arise from some ideal consideration. To think and to act are imperative

in the world of things and persons; both nature and society demand the attention of man, who must receive the world and react upon it. Art and religion, however, are extravagant products of consciousness, since there is nothing in the exterior order which calls upon man to create or to believe. Springing from a form of interior immediacy, art and religion reveal their independence of the facts and activities peculiar to the exterior world; as a result, art is ever threatened by decadence, religion by superstition.

The prime difference between these two forms of characteristic consciousness appears the moment we distinguish between the sense of fulness and that of want. Art may be religious and religion artistic, but when the fundamental note of each is struck, religion shows itself to be possessed of a conscious content in which the sense of insufficiency contrasts with the superabundance peculiar to art. The mere fact of want, however, is not sufficient to constitute the content of religion; this want must be both felt and affirmed. In this spirit of felt and asserted want, Goethe's Tasso said, "Some god gave me power to tell how I suffer." In such a soul-state, there may be psychological material, but would it be sufficient for the psychologist to express this in terms of simple pain? In the character of the 'superior man,' Ernest Hello has said, "The superior man has all the needs of the ordinary man, and he feels these more profoundly than any one else; then, he has other wants." From this point of view, ignoring the assumed aristocracy of the assertion, it may be assumed that religion consists of the power to feel extra-wants—*d'autres besoins*. Such is the nature of the religious content which psychology is called upon to analyze; it is not unearthy, not inscrutable. Capable and deserving of psychological analysis, such characteristic content of consciousness stands in need of and demands a sufficient method of dissection. This analysis is made more than usually puzzling because of the religious tendency to combine self-depreciation with strong self-assertion, although the contradictories noted by Stratton are prepared for just this paradox. In the religious consciousness of Nietzsche's *Zarathustra,* the mingling of strength and weakness is unmistakable, for Nietzsche, like Hello, combined

aristocratic strong-mindedness with deliberate weakness. On the surface, Zarathustra is all strength in his vigorous 'I will;' beneath the surface, he is just as much weakness, and thus he combines the will-to-power with the will-to-suffer. The religious subject, as appears in the instances of Moses and St. Paul, is a Siamese formation of strength and weakness, of anger and humility, whence he is unable to determine whether he is of the bond-woman or the free. The religious self is like the two men who went up to the temple to pray; in one and the same moment, he is self-sufficient and self-depreciating. Because of this twin-tendency, it is necessary for the psychology of religion to take heed of its method, lest its common-sense way of measuring soul-states miss the point altogether.

The contradictory character of religious consciousness puts psychology on its guard against any smug conclusions drawn from a superficial analysis of the religious sense of need. In all genuine religious consciousness, there is a content marked by a sort of Hegelian otherness, a Fichtean positing and oppositing, a Goethean *sterbe und werde*. Like Jacob and Esau, these twins wrestled with each other, even before birth. It is the tendency of the religious consciousness to divide the soul against itself, and leave the subject in an ambiguous condition. The soul of man is batted back and forth as the shuttle-cock. The world is the work of God, yet the world is to be renounced; life is bad, but it must go on forever; all men are sinners, still they must be loved as one loves one's self; man is unworthy, although he is still the object of divine care. In the midst of such complexes, it is vain for psychology of religion to offer as explanation of religious phenomena, such simple data as hunger, fear, wonder and the like, just as in English law legal questions peculiar to the high seas are not to be brought before the ordinary court, but a court of admiralty. Yet, in the midst of this complex and contradiction, the religionist does not claim that his subject-matter is exempt from psychological analysis generally, but that it is to be judged by the proper authority in the proper way; that is to say, the intensive psychologist

must pass upon the characteristic content of the religious consciousness.

The content of religious consciousness evinces three leading ideas: the self, the world, and the Deity. If psychology is in a position to analyze these, then there is such a branch of knowledge as 'psychology of religion;' if it is incapable of such introspection, it must admit its limitations, and devote itself to its proper work. Speaking of the self, religious sentiment and the like, Pillsbury has declared that, "On these problems psychology has nothing to say, since the limitations of its methods and its knowledge give it no right to an opinion."[5] Compared with this strictly scientific view, the psychological work of Stratton reveals the fact that there is still a way of drawing circles around the data of religious consciousness, although such justifiable psychology of religion is far removed from the radical methods of Leuba, who would reduce the ideas of religion to the same level as the ordinary data of consciousness. The conclusion to which the method of the present paper leads is to the effect that, while there is a kind of scientific psychology which is right in remaining silent concerning the content of religion, there is also a humanistic psychology which, without over-stepping its bounds, may make religious consciousness the subject of investigation. As psychologue, if not as psychologist, the investigator may isolate and analyze that which is characteristic in religious consciousness; but if psychology persists in the uncritical employment of naturalistic methods, irrelevancy is bound to creep in and taint the investigation.

As soon as religious consciousness becomes the subject of analysis, it reveals a situation in which the human self is puzzled concerning its attitude toward the obvious natural order about it and a tentative spiritual order which cannot so easily be located or defined. It is commonly assumed that religion begins with the assertion of such spiritual order, usually in the form of the God-idea; but it is just as true to the content of religious consciousness to make an assumption to the effect that the self is equally anxious to detach itself from the immediate world of things. Indeed, the negation of

[5] W. B. Pillsbury, *Essentials of Psychology.* New York, 1911, p. 357.

the natural order is just as germane to religious consciousness as the assertion of a spiritual one, so that a religion may arise and develop without the idea of God, as long as that religion, Buddhism for example, is willing to negate the natural order. Certain religions, as those of Moses and Zarathustra, having distinguished man from the world, are content to preserve a friendly attitude toward the things of nature; others, as Vedanta and Christianity, are inimical to the whole world of things, for which they would substitute a spiritual order of being. The content of religious consciousness is marked by a mood of spiritual self-preservation; Christianity expresses this by declaring that the whole world has not the value of the soul, Vedanta urges the idea that the sum and substance of the world is to be found in the Self (*Khândogya-Upanishad,* VI, 8-16). The 'self' involved in such a spiritual transaction is or is not found by the psychologist, according to whether he follows the methods of the higher or lower introspection. St. Augustine and Pascal have no difficulties with a problem which to Hume and Kant appears insuperable. The scientific psychologist is correct in assuming that he is in no position to handle such data; the religious psychologist is equally in order when he assumes to analyze the very material in question. The matter at hand, far from being unearthy and inscrutable, is simply characteristic; no direct product of the psycho-physical mechanism, it represents the cultural development of the human spirit as such.

Just as religious consciousness finds it possible to identify the idea of selfhood, so it believes itself capable of elaborating an appropriate notion of the world-whole. This latter is accomplished by determining what the world is not rather than by seeking to build up an idea of what the world really is. The world is that which is alien to and rejected by the self in the latter's affirmation of its inner being. Toward the outer world, the attitude of religious consciousness is usually nihilistic. When religious consciousness seeks to formulate the idea of a creator-god, it might seem as though the mind were acting in the interests of the cosmic, but the apparent motive in the midst of this quasi-metaphysical operation is still humanistic, in that the human spirit seems anxious to find

in and behind the world a spirit more akin to its own. Art may rest content with a spiritual treatment of the world according to symbolism, but the usual attitude of religion expresses itself as a desire to banish the world as something antagonistic to the nature and needs of the self. For this reason, certain religions consider their work done when they have reduced the visible world to 'nothing.' This nihilistic attitude appears most strikingly in Tâoism (*Tâo-Teh King,* Pt. I, 1-11) and Buddhism, with its favorite notion of Nirvana. One may not be ready to agree with Nietzsche when he asserts that "in all spiritual religions the nothing is called God," but he can hardly deny that the attempt on the part of the soul may lead to the Vedantist deification of the self or the Taoistic worship of the nought. Only as psychology of religion takes account of such major motives can it hope to avoid the extremes of the inscrutable and the irrelevant.

The mixture of the anthropic and cosmic, the one asserted the other denied, produces the paradoxes often noted in connection with the god-idea in religious consciousness. Such god-consciousness consists of an anthropic being in whom one trusts and to whom one prays, and a cosmic being in whom one believes without necessarily adopting a religious attitude toward it. In referring to theological confusions, Leuba says, "the empirical theologians employ the term 'God' in two different senses." Mankind has ever employed the god-idea in just this dualistic way, hence the contradictions of spiritual life. Far from being so much ignorance on the part of the primitive inhabitant of Israel or India, this dualism shows itself with such an enlightened soul as Pascal, who asserted his belief in God of Abraham and Jacob, rather than the God of the Cartesian philosophers, just as it has more recently shown itself in the religious consciousness of James, who came to the place where he embraced that anthropomorphism and anthropopathism which the 'philosophers' everywhere have ever sought to dismiss. Indeed, with the intensification of life and death during the war, many a person has followed in the foot-steps of the Kaiser, and has prayed to a special God of a particular nation. All of this psychology of religion can explain, even when it will not justify.

Religious consciousness with its anthropic and cosmic foci thus tends to carry along with it a dual intuition of the Divine Being. Such consciousness in its all-too-human behavior seems unable to establish a synthesis of the God believed in and the God worshipped. Just as the ego tends to assert its own being in opposition to the world, so it is just as likely to postulate an anthropic rather than a cosmic notion of the Deity. This contrast, this contradiction must be thoroughly appreciated if one is to handle the data of religious psychology. If the god-idea is purely anthropic, then Leuba is not amiss in inviting theology to become a branch of psychology. If the contradiction between the two gods is destined to continue without criticism or check, then psychology has the right to assume that the religious situation is well nigh hopeless.

But, in the deeper analysis of this human situation, it is well to observe that this conflict of god-ideas is only one, but an excessively trying one, among the many examples of the way in which the human mind refuses to assume a disinterested point of view in its human calculations. Aesthetics and ethics reveal the presence of this over-humanizing, although on a lesser scale; logic and metaphysics are examples of the way in which the human understanding has sought to save itself from being too human. Because of its necessary pretensions to superiority, religion has usually been blamed for a contradiction which is peculiar to all culture, in connection with which man has tried to find refuge in "arguments which the understanding knows nothing about." As far as religion is concerned, it is important to note that the anthropomorphic idea is not the only notion of Godhead which man has entertained. In the midst of his utilitarianism, man has been both able and willing to postulate in his consciousness the idea of a high creator-god, even when he has not always been ready to make use of this notion in his actual worship. But, the psychological fact remains that the idea is there.

Furthermore, a consistent view of religious consciousness of the god-idea reveals the further fact that, while the populace among a people have usually kept the ideas of God anthropic and God cosmic apart and often at variance with each other, religious leaders among such peoples have ever

contended that it were vain to worship the anthropic God in distinction from the cosmic God of belief. In India, the religious teachers whose ideals are recorded in the *Upanishads* urge the people to see that 'true Brahman' is not to be found in any other than a rational manner. In Israel, the prophet warned the people that the God who helped them was indeed 'the Maker of heaven and earth.' Sometimes tolerated for pedagogical purposes, the anthropic God is often held up to ridicule as one with eyes and ears who could neither see nor hear. The education of religious consciousness has usually consisted in an attempt to draw people away from the mere God of worship to the real God of belief; such religious education has been anti-anthropomorphic. In our day of enlightenment, it is to be wondered at and regretted that, instead of continuing to check the anthropic tendency, psychology of religion is so over-humanistic as to aid and abet this natural human weakness. For the preservation and perfection of characteristic religious content, this well-meant interpretation of human religion is not only unnecessary but unwise.

MEMORY FOR ABSOLUTE PITCH

By John Wallace Baird, Clark University

I. Introduction

When a note is struck upon a piano or other musical instrument and the auditors are asked to determine its pitch from hearing alone, one finds that their behavior is of three wholly different sorts: (1) Certain individuals succeed in determining the pitch more or less accurately by a procedure which consists in bringing it into relation with the known pitch of a standard note. (2) A few exceptional individuals are able to identify the pitch immediately, without resorting to any process of comparing or relating. (3) The majority of the auditors, however, are able only to state in the most vague and general terms that the given note is of high pitch or of low pitch, or that it belongs approximately to this or that region of the tonal scale; and if pressed to name the note they can only hazard a hesitant guess, which proves to be wrong in about ninety per cent. of the cases.

Those individuals who are able to identify pitches without having recourse to any process of comparing or relating are said to possess a memory for absolute pitch.[1] Their identifications are instantaneous, requiring but a fraction of a second;[2] these individuals recognize the pitch of a note no less immediately and directly than they recognize the color of a ribbon or the taste of an apple. This capacity is relatively rare; but instances of it are to be found in most communities. A sharp line of demarcation is to be drawn between the capacity possessed by this exceptional group of individuals and the capacity possessed by the group whose procedure consists in relating the given pitch with a known standard. In the latter group the process of relating is accomplished by means of a knowledge of musical intervals. And the standard pitch is either obtained from an external source,—for instance, by striking the *a'* on the piano; or, in cases where the individual has a definite knowledge of his vocal compass, it may be derived from his own vocal apparatus. In order to obtain a note of known pitch, as a point of orientation for his act of relating and identifying, he need but sing the lowest note or the highest note of which he is capable. This type of procedure is available to most students of music; and most cases of alleged memory for absolute pitch prove on examination to be cases in which the pitch of a given note is determined not in an absolute but in a purely relative fashion,—a fact which may be revealed by the individual's habitual tendency to hum or to "feel about with the voice" in his endeavor to assign the given note to its proper place in the tonal series. This tentative humming is never present in a case of genuine memory for absolute pitch. A survey of the literature shows that writers on this topic have frequently failed to differentiate between memory for relative pitch and memory for absolute pitch.

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[1] This term is not wholly free from objection, but so far as the writer knows, no less objectionable term has ever been proposed. See in this connection: J. von Kries, Ueber das absolute Gehör. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane,* III., 1892, p. 257; Otto Abraham, Das absolute Tonbewusstsein. *Sammelbände der internationalen Musikgesellschaft,* Jahrgang III., I., 1901, pp. 1f.

[2] See Abraham, *op. cit.,* pp. 32ff.; see also pp. 48 and 70ff. of this paper.

II. Experiments

The investigation which is here reported was undertaken in coöperation with two members of the faculty of the School of Music at the University of Illinois,—Professor Frederick L. Lawrence, Director of the School of Music and Professor of Music (piano), and Mrs. Constance Barlow Smith, Instructor in Sight-Singing and Ear-Training (in charge of public school methods). The author is under obligation to both of them for helpful criticism and suggestions, as well as for aid in discovering observers who possessed a memory for absolute pitch; he is also indebted to the various observers, mentioned elsewhere, who coöperated in the investigation.[3]

Among the series of experiments which were included in our original plan were the following:

1. The Identification of the Pitch of Notes of Various Clang-Tints

a. Piano Notes. This series of experiments consisted in asking our observers to identify the pitches of notes from the various regions of the piano scale. Before the experiments began we prepared lists in which the eighty-eight notes of the piano were arranged in the order in which they were to be presented for identification. In preparing these lists we endeavored to avoid easy and familiar intervals in consecutive notes, in order that the participation of a knowledge of relative pitch should be eliminated. Three such lists were prepared; and since the notes of each were sometimes presented in a backward order and sometimes in a forward order, we really employed six lists; and it seems probable that no remembrance of the order of presentation could be carried over by the observers from one sitting to another.

[3] The investigation was begun some ten years ago; but the work has frequently been interrupted, at times by the pressure of other duties and at other times by difficulty in securing observers who possessed a memory for absolute pitch. In consequence of this, it has not yet been possible to complete the program as originally planned; but since there seems no prospect of resuming the investigation in the near future, the writer ventures to publish certain of the findings which have already been obtained in the hope that even though incomplete they may throw light upon certain features of this baffling problem.

The observers were asked to specify not only the name of the note (d, a-flat, c-sharp, and the like) but also to indicate the octave to which the note belonged. In order to facilitate the recording of their identifications, a Vergil Practice Klavier was employed throughout,[4]—the observer being asked to indicate upon the keyboard of the klavier the position of the note which she had heard. The observer sat before the keyboard of the klavier, with her back to the experimenter's piano and at a distance of about eight feet from the latter. The experimenter sat before the piano, with a mimeographed copy of one of the six lists of notes upon the music-rack before him; and his procedure consisted in presenting the various notes in successive order, pausing after each presentation until the observer had identified the pitch and had recorded the identification. Above the keyboard of the klavier was fastened a strip of cardboard which extended the full length of the keyboard and which bore the indices of the various octaves (c^5, c^4, etc.) in order to facilitate the observer's designation of the octave to which she assigned the given note. The time required for presenting and identifying a complete list of eighty-eight notes averaged about fifteen minutes, the extreme times being twelve minutes and eighteen minutes.

The observers who took part in this group of experiments (with the number of attempted identifications in parentheses after the name of the observer in each case) were as follows: Miss Augusta S. Cottlow (176); Mrs. Edwin G. Boring (Miss Lucy M. Day, Ph.D., 304); Miss Marie von Engelken (352); Miss Rosa Lee Gaut (817); Miss Bertha I. Howe (264); Miss Mamie Lewis (88); Miss Elva Pease (182); Miss Gertrude L. Peck (229); and Miss Sarah White (425). These nine observers are of course a selected group; they proved to be the only genuine cases of memory for absolute

[4] This Practice Klavier is an instrument which has been devised for the purpose of affording students an opportunity to acquire a playing technique under optimal conditions. The instrument consists essentially of a piano keyboard (eighty-eight keys) of standard size and form, but it has no piano-strings or other internal mechanism of a sound-producing sort. This silent keyboard proved to be peculiarly well-adapted to the purpose for which it was used in the present investigation.

pitch in several score of alleged cases which were brought to the writer's notice. Miss Cottlow is a concert pianist of international reputation; she has been a student of the piano since early childhood and has spent many years of study under the most prominent teachers of Germany and she has subsequently made various tours as a piano virtuoso in America, in England and on the Continent.[5] Miss Howe was, at the time of the investigation, an instructor in piano in the University of Illinois School of Music. Mrs. Boring was a graduate student in psychology in Cornell University. Miss Gaut was a resident of Champaign, Ill. The other observers were students in the University of Illinois School of Music. All of the observers were more or less highly-trained musicians.

b. The Identification of the Pitch of Pipe-Organ Notes. The procedure employed here was essentially identical with that described in the foregoing section, excepting that the strip of cardboard containing the indices of the various octaves arranged in order was here employed alone, without the klavier, since it was not feasible to transport the latter instrument to the auditorium where these experiments were carried through. The sixty-one notes of the organ were arranged in lists in such a sequence as to eliminate the participation of the observer's familiarity with musical intervals. Four qualities or 'stops' of pipe-organ notes were chosen for presentation,—flute, diapason, reed and string, an equal number of each being presented. Five of the observers who had taken part in the piano experiments also took part in the experiments of this section,—*D* (73), *E* (415), *G* (1645), *H* (976) and *W* (415).

c. The Identification of Notes Sung by the Human Voice. In this group of experiments the notes to be identified were sung by four vocalists who had been recommended by the Director of the School of Music,—Miss Lois McCobb, contralto (instructor in voice), Mrs. W. C. Bradford (Miss Florence M. Pruitt), soprano (special student in music), Mr. Leon U. Everhart, tenor, and Mr. George R. Wade, bass.

[5] Miss Cottlow possesses a remarkably clear and definite chromaesthesia (colored hearing) which she has represented in colors for the author, who hopes to publish a detailed description of it in the near future.

Lists of notes which lay well within the compass of the voice of each of these vocalists were prepared in advance, a total of fifty-nine notes ranging from B♭ to g#^{2}; each note was struck upon the piano in a distant room and transmitted, by means of a telephone, to the vocalist who thereupon reproduced it in the presence of the observer. Five of our observers took part in these experiments,—*H* (59), *G* (59), *Pk* (59) and *Ps* (59).

d and *e*. *The Identification of the Pitch of Notes of the Flute and of the Clarinet.* Here a list of notes which had been prepared in advance was presented, by means of the flute and the clarinet, by Mr. Albert A. Harding, a member of the faculty of the School of Music. These notes were selected from the once-accented and the twice-accented octaves. Five observers took part in these experiments,—*H* (26, 26), *G* (26, 26), *Pk* (26, 26), *Ps* (26, 26) and *E* (26, 26).

f. *The Identification of the Pitch of Tuning-Forks.* In this group of experiments the tones of fourteen tuning-forks were presented for identification; the forks were of standard make (König, Kohl) and they were mounted upon resonance boxes. The pitches varied from c to c^3. These were presented in irregular sequence at each of three sittings.

g. *The Identification of Notes beyond the Keyboard of the Piano.* For a description of these experiments see pp. 68f.

2. *Identification-Time*

In this group of experiments we measured the observer's reaction-time in her act of identifying the pitch of a given note. The Hipp chronoscope was employed; the piano key was struck by means of an instrument which closed the electric circuit at the instant when it came in contact with the key, and the observer's reaction consisted in speaking into a voice-key. These time-measurements were made, in adequate number, only in the case of Observer *G*. The averages, presented elsewhere, are computed from 731 reactions to piano notes, and 645 reactions to organ notes. The stimuli, in these reaction-experiments, were distributed uniformly over the various octaves of the keyboard; and the reaction consisted simply in naming the note presented, the reagent here not being required to designate the octave.

3. The Reproducing of Notes by Means of the Tonvariator

In this group of experiments the observer was asked to reproduce the various notes of an octave by means of the *Tonvariator.* The *Variator* was actuated by means of a current of compressed air, of constant pressure. The observer sat with her back to the *Variator.* The experimenter designated any note of the octave, in random sequence; and then proceeded to adjust the instrument, under her direction, varying the pitch in a constant direction until the observer signalled that the designated note had been reached. Determinations were made in both an ascending direction and in a descending direction, twelve determinations being made for each note of the octave.

III. Results

In identifying the note presented, our observers were asked not only to name the note but also to name the octave to which it belonged. A perfect identification, therefore, would be one in which the observer stated the name of the note correctly and also stated the name of the octave correctly. Now it frequently happened,—and this seems to us to be one of the most surprising and significant features of our results,—that the observer succeeded in naming the note correctly but erred in her identifying of the octave to which it belonged. These 'octave-errors' will be discussed in a later section (pp. 69f). In the present section the only errors which concern us are 'note-errors'; for instance, if the observer identified c^3 as c^2 (as she frequently did) it will for our present purposes be regarded as a correct identification.

1. Note-Errors

The numerical results of this part of our investigation are presented in Tables I to VIII. In Table I will be found a record of our observers' percentage of note-error in identifying pitches of the various clang-tints.

a. Individual variation. A reference to this table shows that memory for absolute pitch proves to be a capacity whose perfection varies from individual to individual; and that the limits of this variation are exceedingly wide. Observer *C*

TABLE I

PERCENTAGE OF ERROR IN IDENTIFYING PITCHES OF VARIOUS CLANG-TINTS

Observer	Piano	Organ					Voice					Flute	Clarinet	Forks
		Flute	Diapason	Reed	String	Organ average	Soprano	Contralto	Tenor	Bass	Voice average			
C	1.1	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	33.3
H	3.4	11.9	1.6	3.3	2.1	4.7	12.9	8.3	14.3	38.5	18.5	15.4	30.8	8.3
G	11.1	27.7	22.3	24.5	26.2	25.2	70.6	55.5	14.3	93.3	58.4	30.8	30.8	75.0
L	27.3	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	66.7
D	38.9	.....	.....	.....	.....	68.5	.....	.....	.....	.....	.....	.....	.....	41.4
W	49.4	73.1	58.2	63.5	82.1	69.2	.....	.....	.....	.....	.....	.....	.....	.....
Pk	58.9	.....	.....	.....	.....	.....	88.2	69.2	100.0	20.0	69.4	76.9	100.0	.....
Ps	68.1	.....	.....	.....	.....	.....	88.2	84.6	100.0	100.0	93.2	92.3	92.3	.....
E	74.4	88.6	81.3	87.7	80.1	84.5	88.2	91.7	100.0	100.0	94.9	84.6	84.6	.....

made but 2 erroneous identifications in a total of 176 attempts to identify piano notes,—an average of 1.1 per cent. of error; while Observer *E* made 262 errors in 352 attempts,—a percentage of error amounting to 74.[6] The scores of the other five observers are distributed more or less uniformly between these two extremes,—at the points 3, 11, 27, 39, 49, 59 and 68. The same state of affairs is revealed in the identifying of notes of other clang-tints. For instance, the extreme scores in the case of pipe-organ notes are 4.7 and 84.5, while the intermediate scores are 25.2, 68.5 and 69.2. In the case of notes sung by the human voice the extreme scores are 18.5 and 94.9, while the intermediate scores are 58, 69 and 93. And a similar state of affairs is found to be present in the case of every other clang-tint,—flute, clarinet and tuning-fork.

b. The influence of clang-tint. Accuracy in identification varies with the clang-tint of the note presented for identification. For instance, Observer *H* had a percentage of error of 3.4 in her identification of piano notes; her percentages of error in the identification of pipe-organ notes, voice notes, flute notes, clarinet notes and tuning-fork tones were 4.7, 18.5, 15.4, 30.8 and 8.3 respectively. *G*'s percentages were 11.1 for piano, 25.2 for pipe-organ, 58.4 for voice, 30.8 for flute, 30.8 for clarinet, and 75.0 for tuning-fork. A similar variation of accuracy with clang-tint is found to be present in the case of every other observer. If now we endeavor to arrange the clang-tints in the order of ease of identification, we find that this order varies somewhat from individual to individual; the general averages indicate, however, that the pitch of piano

[6] It is to be noted, however, that our observers were a selected group,—they all possessed some degree of memory for absolute pitch,—and the least successful member of the group represented a degree of efficiency in the identifying of pitches which is far above that of the average member of the human family. If an individual who possesses not the slightest degree of absolute pitch memory had been asked to identify pitches under the conditions of our experiment, his responses (provided he responded at all) would have been nothing more than random guesses. And since in any given case he might respond with the name of any one of the twelve notes of the octave, his guesses would be correct in approximately eight per cent. of the cases; our least successful observer was correct in approximately twenty-six per cent. of her identifications.

notes can be identified more easily than notes of any other clang-tint. Notes sung by the human voice are usually the most difficult to identify. In so far as the averages of our results warrant any seriation in order of increasing difficulty, the order seems to be piano, organ, flute, clarinet, voice; the relative position to be assigned to forks and to certain other clang-tints employed in our experiments is uncertain.

The fact that clang-tint is an influential factor in determining ease of identification of pitch has been reported by other investigators of this topic;[7] and various conjectures have been advanced as to the reason for this difference in difficulty and in accuracy in the identifying of pitches of different clang-tint. For instance, it has been supposed that those clang-tints which are most frequently heard are most easily identified; and that the reason for the difference in ease of identification is to be found in difference in degree of familiarity.[8] But while this hypothesis may furnish a plausible explanation for the relative ease of identifying the pitch of piano notes, which undoubtedly are frequently heard by the average member of modern society, it wholly fails to account for the extreme difficulty in identifying the pitch of the human voice, which is of course heard much more frequently than the piano. Nor does it seem possible to find any correlation between simplicity, complexity or other characteristic of sound-wave on the one hand and ease or difficulty of identification on the other. That identification of pitch is not facilitated by purity of tone is shown by our experiments with the relatively pure tones of mounted forks, which usually proved to be difficult to identify. Even Observer *H* who was much more successful here than any other member of the group was less successful here than in her experiments with piano notes and organ notes.

c. The distribution of errors over the tonal scale. In Table II the record of the errors of the various observers in identi-

[7] For instance, C. Stumpf, *Tonpsychologie,* I., 1883 pp. 235f.; II., 1890, pp. 553f.; von Kries, *loc cit.,* pp. 264 and 269ff.; Abraham, *loc. cit.,* pp. 25ff.

[8] Stumpf, *loc cit.,* I., p. 235; see Abraham's discussion of this point, *loc. cit.,* pp. 25ff.

TABLE II

PIANO ERRORS: THEIR DISTRIBUTION OVER THE TONAL SCALE BY OCTAVES; AND THE DIRECTION OF THEIR DEVIATION

Observer *C*, for instance, had 2 errors in 24 attempts to identify the notes of the contra octave,—a percentage of 8.3. The data at the foot of the table show the direction in which the observers erred,—for instance, in 100 per cent of their malidentifications of the notes of the sub-contra octave they over-estimated the pitch of the given note (identifying a as b-flat or as b, etc.).

Observer	A_2-B_2	C_1-B_1	C-B	c-b	c^1-b^1	c^2-b^2	c^3-b^3	c^4-c^5	Average
C	0	8.3	0	0	0	0	0	0	**1.1**
H	0	5.5	0	2.8	0	2.8	5.5	7.7	**3.4**
G	40.7	26.4	13.3	10.0	5.4	3.5	3.5	13.4	**11.1**
L	100.0	41.7	25.0	25.0	0	16.7	25.0	38.5	**27.3**
D	....	86.4	58.3	21.2	18.7	20.8	27.3	54.2	**38.9**
W	100.0	70.7	65.6	29.8	16.4	18.5	46.2	79.6	**49.4**
Pk	88.9	74.2	64.5	61.3	32.3	41.9	55.2	78.8	**58.9**
Ps	66.7	61.5	66.7	60.0	62.5	70.8	75.0	75.0	**68.1**
E	58.3	85.4	81.2	70.8	64.6	72.9	72.9	77.0	**74.4**
Average	**57.1**	**51.7**	**42.0**	**28.5**	**19.9**	**23.5**	**30.8**	**43.4**	**37.0**
Direction of error:									
Judged too high	100.0	96.8	95.7	80.2	56.7	39.5	26.9	2.1	....
Judged too low	0	3.2	4.3	19.8	43.3	61.5	73.1	97.9	....

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fying piano notes (Table I, second column) has been thrown into a form which shows their distribution over the tonal scale, and the direction of their deviation from correctness. It turns out that the middle section of the tonal scale offers least difficulty in identification,—the average percentage of error in the once-accented octave is approximately 20; while both extremes of the tonal scale offer much greater difficulty, —the percentage of error in the highest octave of the piano keyboard is 43, and in the contra and subcontra octaves, 52 and 57 respectively. The other octaves are distributed more or less uniformly between these two extremes,—the percentage of error as we pass up the keyboard from the contra octave being 42, 29, 20, 24 and 31 (expressed in terms of the nearest whole number). And this state of affairs which is revealed from a survey of the averages of all the errors is found to hold true, completely or approximately, in the case of each individual observer. Whether there is any definite correlation between the position and extent of the region of least difficulty, and the position and extent of the observer's vocal range, is problematic.[9] There is of course a general correlation in that those pitches which the observer can sing are most accurately identified; but there is far from being a perfect coincidence between that region of the tonal scale which can be sung and that region of the tonal scale within which pitch identification is most accurate.

A similar relation appears in Table III which shows the distribution, over the tonal scale, of errors in the identifying of pipe-organ notes. Here we find a tendency toward greater accuracy in the identifying of notes from the middle region of the scale than in the identifying of notes of higher or lower pitch; but the distribution of errors over the tonal scale is less regular here than in the case of piano notes, and it varies somewhat from individual to individual.

d. The distribution of errors over the notes of the octave. Are all of the twelve notes of the octave equally difficult to identify? Or are there certain preferred notes, which prove to be more readily identifiable than other notes? In Tables

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[9] The vocal ranges of our observers are as follows: *C*, a—a^2; *H*, g—f^2; *G*, a—c^3; *L*, g—g^2; *W*, f—a^2; *Pk*, g—$b\flat^2$; *Ps*, e—f^2; *E*, d—b^1.

TABLE III

Pipe-Organ Errors: Their Distribution Over the Tonal Scale by Octaves; and the Direction of Their Deviation

The results are recorded in percentages,—Observer *H*, for instance, had 7 errors in 48 attempts to identify the "flute" notes of the lowest octave, a percentage of error amounting to 14.6. The data at the foot of the table indicate the direction in which the observers erred,—for instance, in 96.7 per cent of their erroneous identifications of notes of the lowest octave they over-estimated the pitch.

Observer		C-B	c-b	c^1-b^1	c^2-b^2	c^3-c^4	Aver.
	Flute	14.6	6.3	4.2	14.6	19.2	**11.9**
	Diapason	2.1	0	0	2.1	3.1	**1.6**
H	Reed	2.1	0	2.1	2.1	9.3	**3.3**
	String	0	0	2.1	4.2	3.5	**2.1**
	Average	**4.7**	**1.6**	**2.1**	**5.7**	**9.1**	**4.7**
	Flute	48.3	23.2	14.7	14.9	24.5	**27.7**
	Diapason	28.2	22.1	22.4	16.4	21.3	**22.3**
G	Reed	21.1	21.8	29.7	25.0	25.5	**24.5**
	String	28.6	38.2	26.7	17.8	20.6	**26.2**
	Average	**31.6**	**26.3**	**23.4**	**18.5**	**23.0**	**25.2**
	Flute	95.0	77.8	50.0	65.0	47.6	**73.1**
	Diapason	81.5	50.0	28.6	60.7	70.0	**58.2**
W	Reed	75.0	72.2	31.6	80.0	66.7	**63.5**
	String	100.0	75.0	72.2	83.3	77.3	**82.1**
	Average	**85.6**	**67.3**	**43.8**	**70.4**	**65.9**	**69.2**
	Flute	86.9	90.0	90.0	90.5	87.5	**88.6**
	Diapason	89.5	94.1	81.9	61.6	70.0	**81.3**
E	Reed	86.9	90.0	89.5	90.5	82.6	**87.7**
	String	86.9	81.8	80.9	77.3	80.9	**80.2**
	Average	**87.5**	**88.6**	**85.4**	**80.0**	**80.7**	**84.5**
	Grand aver.	**52.4**	**46.0**	**38.7**	**43.7**	**44.7**	....
Direction of Error	Judged too high	96.7	81.5	51.9	37.8	7.6	....
	Judged too low	3.3	18.5	48.1	62.2	92.4	....

IV and V the record of errors for piano notes and organ notes is compiled in such fashion as to show their distribution by notes. Table IV shows, in its first column, the percentage of error made by each observer in all her attempts to identify the pitches of the eight c's of the piano keyboard, while the second, third and other columns of the table record similar data for the c-sharps, the d's and other notes of the octave. Table V records the note-distribution of errors made in the organ experiments.

A survey of these tables shows that the naturals* are usually identified with greater accuracy than the accidentals,—the average percentage of error being 30 for the naturals, and 40 for the accidentals in the case of piano notes, while the percentages are 41 and 50 in the case of organ notes. There are, however, notable exceptions to this general rule. *G*'s most difficult organ note was a natural (d, 38 per cent. of error) and her easiest organ note was an accidental (a-flat, 9 per cent. of error); indeed, her average percentage of error for the sharps and flats was somewhat less than her average percentage of error for the naturals,—22 per cent. as compared with 27 per cent. And we find that a natural, if not the most difficult note, is at least in the most difficult group of notes for both *C* and *L*.

Our tables show that every observer is more successful in identifying certain notes of the octave than in identifying other notes of the octave. In the experiments with piano notes *Pk* never succeeded in identifying any of the d's, but she failed in only one-third of her attempts to identify the c's. *L* never failed to identify the c's and the d's, but she failed in one-half of her attempts to identify the a's. *H* invariably succeeded in identifying the c-sharps, the d's, the e's, the f's, the f-sharps and the b's, but she frequently had difficulty with the e-flats. *G* never failed with the f's and failed only once with the e's, but she had 19 errors with the b-flats. *D* had only 11 per cent. of error in her attempts to identify the f's, while she had 75 per cent. of error in her identifications of the b-flats.

* Here and elsewhere in this paper we shall, for the sake of brevity, employ the term naturals to indicate the white notes and accidentals to indicate the black notes on the keyboard of the piano or organ.

TABLE IV

Piano Errors: Their Distribution by Notes

This table shows the percentage of erroneous identifications of each note of the octave. For instance, Observer *H* had 1 error in her 24 attempts to identify the various c's of the keyboard,—a percentage of error amounting to 4.2.

Observer	c	c#	d	e♭	e	f	f#	g	a♭	a	b♭	b
C	0	0	0	0	7.1	0	0	0	7.1	0	0	0
H	4.2	0	0	14.3	0	0	0	4.8	4.8	9.6	4.8	0
G	7.0	8.7	7.7	9.5	1.6	0	17.7	14.1	10.3	9.7	30.1	13.9
L	0	42.9	0	28.6	14.3	14.3	14.3	14.3	28.6	50.0	50.0	62.5
D	20.0	47.6	21.5	78.6	47.6	10.7	33.3	33.3	62.5	45.8	75.0	20.8
W	35.3	82.5	32.4	53.8	46.9	50.0	63.6	29.1	36.4	58.8	44.5	52.2
Pk	33.3	70.6	100.0	66.7	35.0	38.9	61.1	58.9	61.1	45.6	81.0	57.2
Ps	68.7	60.0	42.9	60.0	71.4	84.6	71.2	50.0	92.3	55.6	88.2	62.5
E	56.3	96.4	35.7	89.3	96.4	57.1	85.7	60.7	89.3	75.0	87.5	63.6
Average.	**24.5**	**42.9**	**24.9**	**39.2**	**31.0**	**23.6**	**39.6**	**26.8**	**34.5**	**34.5**	**45.9**	**36.9**

TABLE V

Pipe-Organ Errors: Their Distribution Over the Octaves by Notes

This table shows the percentage of error in identifying each of the twelve notes of the octave. In the case of the "flute" notes, for instance, Observer *H* had two errors in her 24 attempts to identify the various c's (C, c, c^1, c^2, c^3, and c^4),—a percentage of 8.3.

Observer		c	c#	d	e♭	e	f	f#	g	a♭	a	b♭	b
	Flute	8.3	30.0	5.0	15.0	5.0	5.0	5.0	0	25.0	20.0	20.0	5.0
	Diapason	0	5.0	0	0	0	0	0	5.0	0	5.0	5.0	0
H	Reed	0	0	0	0	0	0	0	0	10.0	5.0	25.0	0
	String	0	0	0	0	0	0	0	0	5.0	5.0	15.0	0
	Average	**2.1**	**8.7**	**1.3**	**3.8**	**1.3**	**1.3**	**1.3**	**1.3**	**10.0**	**8.8**	**16.3**	**1.3**
	Flute	26.3	22.2	38.2	28.6	22.2	14.3	19.4	37.9	12.9	29.6	33.3	24.1
	Diapason	15.0	12.1	33.3	32.4	29.0	21.9	9.4	20.6	11.1	16.7	36.7	29.0
G	Reed	26.8	37.5	43.2	21.6	23.3	8.8	14.3	25.7	10.0	25.0	33.3	33.3
	String	17.5	20.6	37.1	33.3	31.4	12.1	11.4	31.6	3.2	28.6	41.2	42.4
	Average	**21.4**	**23.0**	**38.1**	**28.9**	**26.8**	**14.2**	**13.6**	**28.7**	**9.2**	**24.8**	**35.6**	**32.5**

TABLE V—*Continued*

PIPE-ORGAN ERRORS: THEIR DISTRIBUTION OVER THE OCTAVES BY NOTES

Observer		c	c#	d	e♭	e	f	f#	g	a♭	a	b♭	b
W	Flute	44.4	100.0	85.6	50.0	66.7	75.0	55.6	55.6	100.0	62.5	57.2	87.5
	Diapason	23.0	84.6	72.7	53.8	41.7	66.7	90.9	38.5	55.6	33.3	77.8	69.2
	Reed	11.1	100.0	50.0	55.6	87.5	83.3	80.0	28.6	71.4	57.2	66.7	85.6
	String	75.0	85.6	87.5	100.0	100.0	100.0	90.0	42.9	88.9	50.0	75.0	85.6
	Average	**35.9**	**91.4**	**73.5**	**61.4**	**70.3**	**79.1**	**80.0**	**41.7**	**79.1**	**48.6**	**70.0**	**80.0**
E	Flute	100.0	100.0	66.7	100.0	85.6	75.0	100.0	60.0	90.0	90.0	88.9	100.0
	Diapason	75.0	84.6	42.9	87.5	100.0	71.4	100.0	66.7	80.0	90.0	100.0	75.0
	Reed	100.0	100.0	55.6	100.0	85.6	90.0	100.0	44.4	100.0	100.0	100.0	80.0
	String	90.0	83.3	44.4	100.0	75.0	44.4	100.0	85.6	100.0	62.5	83.3	88.9
	Average	**92.3**	**91.2**	**55.6**	**96.9**	**86.7**	**70.6**	**100.0**	**37.1**	**91.9**	**86.5**	**93.8**	**86.1**
	Grand average	**37.9**	**53.6**	**42.1**	**47.8**	**46.3**	**41.3**	**48.7**	**27.2**	**47.8**	**42.0**	**53.9**	**50.0**

The same state of affairs is found in the case of the organ notes. *E*'s identifications of the g's were more than twice as accurate as her identifications of the c's, the e's, and the b's. *G* had approximately four times as many errors with the b-flats as with the a-flats. And *W*'s identifications of the c's were approximately twice as accurate as her identifications of the d's, the e's, the f's and the b's.

For each individual, then, there are certain relatively easy notes and certain relatively difficult notes; and this phenomenon of preference appears both in the identification of piano notes and in the identification of organ notes. But there is a general lack of coincidence between the note which is preferred in the case of piano clangs and in the case of organ clangs. It is true that for *E* the d's and the f's are among the easiest notes in both cases, while the b-flats, the e-flats and the f-sharps are among her most difficult notes in both cases; the f's and the c's are relatively easy for *G* in both cases while the b's and the b-flats are relatively difficult; c is relatively easy for *W* in both cases while c-sharp and f-sharp are relatively difficult; and there are other coincidences of this general sort. But if for each observer we arrange the twelve notes of the octave in order of difficulty as regards piano and as regards organ, we find that there is a striking lack of conformity in our two lists. For example, in the case of organ notes, a stands in the first quartile of *W*'s list, while for the organ notes it stands in the last quartile. And in *C*'s lists we find that c-sharp shows a similar shift of position.

Nor is there any unanimity among the observers as to which note of the octave is most accurately identifiable either in the case of piano notes or of organ notes. It is true that f proves to be the easiest note, or at least to be a member of the easiest group of notes, for four of the nine observers; and it stands in the first quartile for three other observers. But it stands in the second half of the list for the remaining two observers, —in the last quartile for one of them. And while d is the easiest note of the twelve for two observers, and in the easiest group for two others, it is the most difficult note of the twelve for one of the observers. A similar lack of agreement is present in the case of organ notes. One observer finds that

d is the most difficult note of the twelve, while two others find that it is relatively easy; b is easy for one but difficult for two others; g proves to be easy for three observers but relatively difficult for the fourth. The most difficult note of the twelve is f-sharp for one observer, c-sharp for another, d for another, and b-flat for the fourth.

The averages of all observers show that f and c are most accurately identified in the case of piano notes and g and c in the case of organ notes; while c-sharp and b-flat are the least accurately identified in both cases.

e. The direction of deviation. The data at the foot of Tables II and III show, for each octave, the percentage of cases where the mal-identification erred in the direction of an overestimation of pitch, and in the direction of an underestimation of pitch. These data reveal the presence of a central tendency of judgment,—the pitch of low notes tends to be judged too high, and the pitch of high notes tends to be judged too low. A second general tendency manifests itself in the finding that overestimations of pitch are relatively more frequent (57.1 per cent.) in the identifying of piano notes, while underestimations of pitch tend to be relatively more frequent (58.7 per cent.) in the identifying of organ notes.

f. The magnitude of error. The errors varied in magnitude from one semitone to two octaves. Apart from the illusion of the octave,[10] however, the magnitude rarely exceeded three semitones excepting in the case of four of our nine observers. The proportion of grosser errors increases with decrease of accuracy in estimating pitch (Table VI). Eighty-seven per cent. of all the piano errors made by the three most successful observers were errors of small magnitude (one semitone), while only 43 per cent. of the errors of the three least successful observers were errors of small magnitude; and the same relation obtains in the case of organ notes. When the task becomes more difficult, as in the identifying of organ notes, not only do the errors become more numerous but they also increase in magnitude. This principle is especially evident in the case of the more successful observers, where the percentage of larger errors (two or more semitones)

[10] See pp. 69ff.

TABLE VI

THE RELATIVE FREQUENCY OF ERRORS OF VARIOUS MAGNITUDES

The results are expressed in per cents,—for instance, 78 per cent of *H's* errors in identifying the pitch of piano notes were errors of one semitone, 11 per cent were errors of two semitones, and 11 per cent were errors of more than two semitones, etc.

Observer	Piano Errors			Organ Errors		
	Errors of one semitone	Errors of two semitones	Errors of more than two semitones	Errors of one semitone	Errors of two semitones	Errors of more than two semitones
C.........	100	0	0	......	......	......
H..........	78	11	11	53	22	25
G..........	83	10	7	70	19	11
L..........	38	29	33	......	......	......
W..........	33	21	46	43	18	39
Pk.........	42	18	40	......	......	......
Ps..........	52	22	26	......	......	......
E..........	34	23	43	24	21	55
Average.......	**58**	**16**	**26**	**49**	**20**	**31**

increased from 19 per cent. of the total number of errors, in the case of piano notes, to 39 per cent. in the case of organ notes.

g. Most frequent confusions. If we examine the errors with a view to determining whether there are any notes which are especially subject to being confused with each other,—and which may therefore be presumed to be especially similar in sound,—we find that there are certain errors which recur over and over again in our records; and that certain observers are especially prone to certain errors. For instance, our records show that on four occasions *H* identified $c\#^2$ as $a\flat^2$ (organ; flute, string). This is a most unexpected confusion. No other observer ever confused any c# with any a♭; *H*'s identifications of c^2, $c\#^3$ and $c\#^4$ were invariably correct, but she twice

identified a♭ as c# (organ; flute, diapason). The cases in which any given pair of piano notes was most frequently confused with one another by any individual were as follows: *W* confused b-flat and e-flat 9 times. This confusion was always made in one direction, e-flat being identified as b-flat but b-flat never being identified as e-flat; this pair was confused only once, and in the opposite direction, (*H*) by all other observers. (See next paragraph.) *W* confused b-flat and c-sharp 7 times, all other observers only twice. *E* confused e-flat and a-flat 5 times; this confusion was never made by any other observer. Certain confusions were common to more than one observer: c and g were confused 7 times by E, 6 times by *W*, 5 times by *Pk*, but never by any other observer; c-sharp and e-flat were confused 5 times by *W* and 4 times by *Ps*, never by any other observer; e and g were confused 7 times by *W*, 6 times by *E*, 4 times by all other observers.

The most frequent confusions of piano notes with the total number of times they recurred in the identifications of all observers are as follows: c and g, 18 times (c being identified as g, 12 times; and g as c, 6 times); e and g, 17 times (14+3); b and g, 13 times (9+4); f and g, 11 times (9+2); g and d, 10 times (8+2); e-flat and b-flat, 10 times (9+1); a and c, 10 times (10+0); c-sharp and e-flat, 9 times (9+0); c-sharp and b-flat, 9 times (7+2); b-flat and g, 9 times (6+3); e-flat and a-flat, 7 times (5+2); b and d, 8 times (6+2); e-flat and a, 7 times (4+3).

The most striking confusion of organ notes occurred in the case of b-flat and e-flat; b-flat was identified as e-flat 15 times by *H* and 5 times by *W*, never by any other observer, and e-flat was never identified as b-flat by any observer. It will be remembered that *H* and *W*, but no other observer, confused these two notes in the piano experiments.[11] It seems worthy of mention in this connection that on several occasions *Pk* volunteered the remark that e-flat and b-flat sounded very much alike; and she added that these two differed from all other notes of the octave in that both of them were peculiarly

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[11] Of these 15 confusions, 4 occurred with string stop, 6 with reed stop, 4 with diapason, and 1 with flute; the errors were distributed by octaves as follows: b♭, 2; b♭^{1}, 3; b♭^{2}, 6; b♭^{3}, 4.

"soft and mellow." Yet our records show that *Pk* never identified e-flat as b-flat, nor b-flat as e-flat. It is also to be noted that *E* identified e as g, 9 times and *W* identified e as g, 6 times; *H* and *G* never failed to identify these notes correctly, and *E* and *W* never identified g as e.

The most frequently confused organ notes were as follows: e-flat and c-sharp, 30 times (21+9); b and d, 24 times (14+10); b-flat and e-flat, 20 times (20+0); e and g, 15 times (15+0); b and g, 14 times (8+6); e-flat and a-flat, 13 times (7+6); c and g, 12 times (7+5); c and e, 11 times (8+3); b-flat and g, 11 times (8+3); b-flat and e, 9 times (8+1); a and c, 9 times (6+3); c-sharp and a-flat, 8 times (5+3); d and g, 7 times (5+2).

If, now, we arrange these pairs of confused notes in order of frequency of confusion in the piano experiments, and if we make out a similar list showing the relative frequency of confusion of the various pairs in the organ experiments, we find that c and g is the most frequently confused pair of notes in the piano experiments and that this pair stands seventh in order of frequency in the organ experiments; the e and g pair comes second in the piano list, fourth in the organ list; b and g comes third in the piano list, fifth in the organ list. That is, the three pairs whose members are most frequently confused with one another in the piano experiments stand among the first seven of the most frequently confused pairs in the organ experiments. It is a remarkable fact that g is a member of all three of these pairs. Indeed, g is a member of each of the first six pairs of the piano list;[12] but it is present only twice in the first six pairs of the organ list. This would seem to indicate that g is much more readily identifiable when presented in organ clang-tint than when presented in piano clang-tint.

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[12] This fact seems surprising in view of the data presented in Table IV, where g proves to be one of the easiest notes of the octave to identify,—the averages of Table IV show that only three other notes of the twelve are more readily identifiable than g. But an examination of the data which appears within the parentheses on the present page shows that the vast majority of these confusions in which g played a part were not cases in which g was mistaken for another note but cases in which other notes were mistaken for g.

If we add the number of times that each of these pairs was confused in the piano experiments and the number of times of confusion in the organ experiments, we obtain the following totals: e-flat and c-sharp, 39 times (21+18);[13] e and g, 32 times (29+3); b and d, 32 times (20+12); b-flat and e-flat, 30 times (21+9); c and g, 30 times (19+11); b and g, 27 times (17+10); b-flat and g, 20 times (14+6); e-flat and a-flat, 20 times (12+8); a and c, 19 times (16+3); g and d, 17 times (10+7).

When we classify these most frequently confused pairs of notes, employing as our basis of classification the degree of community of overtones or of musical relationship existing between the two members of the pair, we find the fifths were confused in a total of 51 cases,—31 times in the piano experiments and 20 times in the organ experiments; the corresponding data for the other musical intervals are: fourths, 54 times (14+40); thirds, 80 times (28+52);[14] sixths, 70 times (38+32).

One would expect, on *a priori* grounds, that liability to confusion would be a function of community of overtones; and that notes would be subject to being confused with one another in proportion as they possess overtones in common, or in proportion as their relationship is such as to give rise to a relatively perfect fusion with one another.[15] But the reverse proves to be true. The findings reported in the preceding paragraph do not furnish complete data as to the relative frequency of confusions of seconds, sevenths, and tritones; but they do show that notes between which the relationship of the fourth obtains are more frequently confused with one another

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[13] As indicated in a preceding paragraph, these symbols mean that e-flat was identified as c-sharp 21 times, and that c-sharp was identified as e-flat 18 times.

[14] There were 24 cases of confusion of the major thirds and 4 confusions of minor thirds in the piano experiments; 14 confusions of major thirds and 38 confusions of minor thirds in the organ experiments. Major sixths were confused 15 times, and minor sixths 23 times in the piano experiments; major sixths were confused 21 times, and minor sixths 11 times in the organ experiments.

[15] Especially in view of the fact that octave-confusions are so frequent; see pp. 69ff.

than are notes between which the relationship of the fifth obtains; and that thirds and sixths are confused much more frequently than fourths or fifths. We have already pointed out that errors of a semitone constitute a very considerable proportion of the total number of errors made by our observers,—a fact which further confirms our paradoxical finding that those tones are most subject to confusion which are least subject to tonal fusion. This finding is of course not surprising in the case of errors which reach a magnitude of only one or two semitones, for it is rather to be expected that observers should frequently fail to differentiate between notes which lie at adjacent points upon a continuously graduated series. But it is a remarkable and significant fact that in such a considerable number of cases those notes which are subject to confusion by individuals who possess a memory for absolute pitch should prove to be notes which are so clearly and so readily differentiated by individuals whose reactions to tones are determined largely by a memory for relative pitch.

Several of our observers reported that the sharps and flats possess a peculiar sound-quality which differentiates them from the naturals; and on various occasions they remarked that they had recognized that the given note was a natural (or an accidental) before they had recognized which natural (or accidental) it was. This belief in the existence of a specific accidental-quality which differs from the specific natural-quality seems to be widespread among individuals who possess a memory for absolute pitch. When asked to describe these two qualitative characteristics they find it impossible to find words which can do justice to the minute nuances of sound-quality involved in the differentiation; and they usually have recourse to metaphor. "The naturals are cool while the accidentals are warm" is the way in which one observer expressed the distinction; another reported that the accidentals are brilliant, the naturals dull.

Now if this peculiarly elusive criterion were of service to the observers in our experiments, one would expect to find that in cases of erroneous identification the observers would tend to confuse naturals with naturals and accidentals with accidentals rather than to confuse naturals with accidentals.

Accordingly we have prepared a tabulated statement of the percentages of errors (Table VII) in such form as to show the relative frequency of these various sorts of confusions. These data show that the most frequent type of error consists in identifying accidentals as naturals,—that is, in reporting that a given accidental is a natural; and that the least frequent type of error consists in confusing one member of the accidental group with another member of the same group. This tendency is common to all of the observers; indeed, two of the observers recognized in only about five per cent of the cases that the (erroneously identified) accidentals were accidentals. And the averages of the eight observers show that in more than half of their mal-identifications they failed to recognize that accidentals were accidentals and that naturals were naturals, —that is, they reported that the given stimulus belonged to a group which is alleged to be qualitatively different from the stimulus; and in less than half of their erroneous identifications did they assign the stimulus to the group with which it

TABLE VII

This table shows the relative frequency with which, in identifying the given note erroneously, the observers made the various sorts of accidental-confusions and natural-confusions. For instance, in 44.4 per cent of *H's* erroneous identifications, accidentals, presented by the experimenter, were identified as naturals, etc.

Observer	Accidentals mistaken for naturals	Naturals mistaken for accidentals	Naturals mistaken for naturals	Accidentals mistaken for accidentals
H	44.4	22.2	22.2	11.1
G	54.3	27.6	12.8	5.3
L	32.0	32.0	16.0	20.0
D	31.7	22.4	27.3	18.6
W	27.5	18.9	34.2	19.4
Pk	37.6	20.3	36.7	5.4
Ps	35.5	24.6	28.6	11.3
E	35.7	15.3	35.7	13.3
Average....	**38.0**	**20.1**	**29.8**	**12.1**

is alleged to be homogeneous. In view of these facts it is impossible to accept the statement that all accidentals are perceptibly similar to one another, and perceptibly different from all naturals.

h. Errors in the identifying of pitches beyond the range of the piano. Our findings had shown that the possessor of a memory for absolute pitch can readily and correctly assign a given pitch to its proper place within the octave, *i. e.,* can name the note; but that she frequently hesitates and frequently errs in her attempt to name the octave to which this note belongs. This remarkable finding, together with such descriptions of mental procedure as the observers were able to furnish, raised the question as to whether one might assume that these observers find in each of the notes of the octave a certain *quale,*—a c-ness, a d-ness, etc.; and whether in consequence of this peculiar characteristic each note differs from its fellows within the octave but resembles the corresponding note of other octaves. This conjecture led to an additional series of experiments in which we employed the Galton Whistle and the König bars. The series of notes presented by means of the König bars were eight in number,—e^5, g^5, c^6, e^6, c^7, e^7, g^7, and c^8. These eight notes were each presented three times, in irregular sequence. Observer *G* reported that in all of the notes beyond c^7 the pitch was so indefinite and so completely smothered by noise as to be incapable of identification. In the 15 identifications which she attempted she had 10 errors, a percentage of error amounting to 66.7. *C* also failed to perceive any distinct pitch in any note above c^7; she had 5 errors in her 15 identifications, a percentage of 33.3. *H* attempted to identify the pitch in twelve cases and she had but 3 errors, a percentage of 25.0.

In the series with the Galton Whistle we presented the fifteen naturals extending from d^5 to d^7; these were presented, in irregular order, at each of three sittings. *G* usually reported that the sound of the two highest members of the series was not sufficiently clear to be identified. She had 11 correct judgments in the 41 cases where she attempted an identification, a percentage of error amounting to 73.2. *C* also found that certain of the higher pitches were unrecogniz-

able but she attempted 39 identifications with a percentage of error amounting to 41.0. *H*'s percentage of error in 33 attempts was 51.6.

2. Octave Errors

One of the most striking features of our findings is a phenomenon which may be called the illusion of the octave. It frequently happens that an observer who possesses absolute pitch memory is able to name a given note correctly but fails to designate the octave to which the note belongs. As we point out elsewhere, the process of identifying the pitch of any given note is made up of two distinct stages: An initial stage, which consists in a prompt naming of the note; and a subsequent stage, in which the observer names the octave to which the note belongs. In many instances, the observer is able to name the octave only after devoting an appreciable time to a process of deliberating and groping. The note-response and the octave-response were usually separated by an appreciable interval; and the degree of subjective assurance attaching to the octave-identification was usually very much less than that attaching to the note-identification. The octave-error was common to the identifications of all nine of our observers; and it was present in the identifying of notes of every variety of clang-tint.

An examination of our records shows that the octave-error occurred with the following frequencies: In 5.1 per cent of the cases where Observer *C* designated the (piano) note correctly, she designated the octave incorrectly; *H,* 40.8 per cent; *G,* 27.6; *L,* 7.9; *D,* 12.9; *W,* 13.6; *Pk,* 6.9; *Ps,* 11.4; *E,* 2.5. The illusion was somewhat less frequent in the identifying of organ notes,—the percentages here being: *H,* 31.5; *G,* 21.7; *W,* 10.3; *E,* 3.2. These data show that the illusion of the octave is of frequent occurrence, that it is subject to individual variation, and that it varies with the clang-tint of the note presented. It is usually more frequent in the more accurate observers, although we have in the case of *C* a notable exception to this general principle; and it is more frequent with relatively easy clang-tints. This illusion is undoubtedly a phenomenon which is especially characteristic of memory

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for absolute pitch as compared with memory for relative pitch. The individual who estimates the pitch of a given note in relation to a standard is characterized by the presence of note-errors alone; he has no difficulty in locating the general region of the scale to which the given pitch belongs. And it seems a remarkable phenomenon that in the possessor of absolute pitch memory we have an individual who is especially successful in assigning the given note to its position within the octave, but is frequently at a loss as to the general region of the scale to which the note is to be assigned.

3. Identification-Time

The naming of the note was usually an instantaneous process. It is true that the observer hesitated in certain instances; but her identifications were almost invariably erroneous when they were not made with the utmost promptness. The hesitations were more frequent with notes of very high pitch and very low pitch, and with notes of difficult clang-tints. One observer stated: "As soon as I hear the note I know its name. If I do not recognize it immediately I am lost; but I usually require some little time to discover the octave to which it belongs." And this statement describes the typical reaction of all observers.

In order to determine the relation between immediacy and accuracy, we arranged a series of experiments in which organ notes were presented in rapid succession in one case, and with pauses intervening between presentations in the other case. In the former case the observers were instructed to record their decisions as rapidly as possible, and as a matter of fact, if they hesitated they found that the next note had been presented before they had recorded their identification of the preceding note; in this series the notes were presented at the rate of about ten per minute. In the slow series the observers were instructed to take as much time as they required for accurate identification, and no note was presented until its predecessor had been identified and recorded by the observer; the rate of presentation in this series was about one per minute. It turned out that every observer identified the rapid series more accurately than the slow series, the

average percentage of error being 41.8 for the rapid series and 49.6 for the slow series. The data for the individual observers were: *H,* 3.3 per cent and 6.1 per cent; *G,* 24.4 and 27.6; *W,* 59.1 and 75.5; *E,* 80.3 and 89.3.

In numerous instances the observers, in the slow series, were found to be engaged in a process of humming and feeling about with the voice in an endeavor to identify a particularly difficult note.[16] Yet notwithstanding their recourse to this additional expedient, and notwithstanding the fact that

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[16] This was clearly an employment of the relative pitch criterion. That this criterion may be employed successfully, but that it was not employed in the ordinary identifications of our observers is shown by the following incidents:

1. When in her regular series an observer made an erroneous identification of any note, it was the custom of the experimenter to proceed as follows: Instead of then presenting the note which stood next in order upon his mimeographed list, he introduced an extra note of such pitch that it was separated from the erroneously identified note by some such easy and familiar interval as an octave or a fifth. This expedient was employed in the hope that if the observer were judging in terms of her 'sense of interval' she would betray herself by now committing a second error of the same magnitude and direction. In not a single instance did we succeed in trapping one of our observers by this device.

2. In additional experiments notes were identified by means of relative pitch alone. Miss Winifred Forbes, instructor in violin, Mr. George F. Schwartz, instructor in violin, and Director Lawrence,—all of whom possessed a highly-developed 'sense of interval',—were kind enough to serve as observers in these experiments, the detailed results of which can not be discussed here. We employed the same lists of notes as in our other experiments. But here, in our experiments with Miss Forbes and Mr. Schwartz, each note was preceded by an a′ struck upon the piano, and the observer was instructed that the pitch of the note which followed was to be identified in relation to the a′; while in the experiments with Director Lawrence we first made an accurate determination of his vocal compass and then he was instructed that the pitch of the given note was to be estimated by means of his voice alone. The percentage of error varied, in the three observers, between 12 and 37,—which shows that relative pitch suffices for identification provided appropriate conditions are furnished. It turned out, however, that the procedure of these observers was wholly different from the procedure of the observer who possesses a memory for absolute pitch; and the identification-time, instead of being a fraction of a second, frequently amounted to several minutes.

in the rapid series there were occasional blanks in the record on account of the observer's lack of sufficient time for recording her identification, the identifications were considerably more accurate in the rapid series.

In our measurements of the identification-time,[17]—which were sufficiently numerous to be of value only in the case of Observer G,—we obtained the following results (all of these results are expressed in terms of sigma): The average identification-time, for piano notes (731 reactions) was 754.4 ± 187.3. The times, averaged separately for each octave of the piano, were: A_2—B_2, 1241.3 ± 274.1; C_1—B_1, 957.7 ± 205.5; C—B, 798.8 ± 184.9; c—b, 726.5 ± 153.1; c^1—b^1, 658.3 ± 127.0; c^2—b^2, 615 ± 133.3; c^3—b^3, 636.3 ± 151.8; c^4—c^5, 765.2 ± 209.5.

G's average identification-time for organ notes (645 reactions) is somewhat longer, 957.8 ± 204.9. The average times for the four stops were: Flute, 1078.9 ± 226.4; diapason, 822.6 ± 189.6; reed, 993.0 ± 219.5; string, 935.3 ± 183.9.

A survey of these data shows that the pitch of piano notes is identified more promptly than the pitch of organ notes; that the identification of pitches of the central region of the scale requires less time than the identification of very high or very low pitches; and that there is a fairly close correspondence throughout between accuracy of identification and promptness of identification.

4. The Reproducing of Designated Notes

Six of our observers were asked to reproduce each of the notes of an octave, by means of the *Tonvariator*. The results of these experiments are given in Table VIII. A survey of this table shows wide individual differences in accuracy of reproduction,—the degree of accuracy of any given individual corresponding fairly well with her degree of accuracy of identification as recorded in Table I. Indeed, in certain instances it was found that in the case of those individuals who had proved to be least accurate in their identifications, the notes reproduced upon the *Tonvariator* represented broad bands of the tonal region; and these bands were sometimes

[17] See p. 48.

TABLE VIII. THE REPRODUCING OF DESIGNATED PITCHES, BY MEANS OF THE STERN TONVARIATOR

OBS.	C			H			G			Pk			Ps			E		
	Extremes	Average	M. V.	Extremes	Average	M. V.	Extremes	Average	M. V.	Extremes	Average	M. V.	Extremes	Average	M.V.	Extremes	Average	M. V.
e^2	....	......	.....	640 655	647.8	3.7	642 662	649.5	4.2	627 680	650.8	10.7	636 691	646.3	9.1	604 662	631.5	14.7
f^2	....	......	.....	678 690	684.5	4.3	674 698	687.0	5.6	682 725	701.5	8.5	676 765	694.3	22.3	630 712	665.3	24.6
g^2	....	......	.....	765 785	771.9	6.8	742 799	770.3	11.5	757 798	773.3	9.1	755 825	802.8	11.6	740 817	769.0	18.4
a^2	864 885	876.3	6.1	864 872	869.3	2.1	840 878	865.6	7.2	846 894	870.6	9.7	813 876	843.4	13.5	830 902	862.3	28.7
b^2	970 983	977.5	4.3	966 985	972.7	5.8	940 982	965.6	13.1	938 998	962.5	11.6	925 1014	959.3	18.7	910 1023	952.5	29.6
c^3	1034 1041	1037.4	2.2	1020 1056	1035.4	9.1	998 1074	1028.5	14.8	1018 1070	1037.9	8.5	995 1085	1046.3	20.5	1002 1067	1029.3	11.6
d^3	....	......	.....	1128 1161	1138.0	11.7	1136 1180	1159.8	5.7	1127 1172	1141.5	11.3	1112 1171	1140.0	14.6	1136 1195	1161.5	18.3

so broad that the zones of neighboring notes overlapped. For instance, in one of her attempts to 'find' the note e^2, *Ps* reported that a stimulus of 691 vibrations per second seemed to her to be e^2, while on another occasion she reported that a somewhat lower pitch, 676 vibrations, was f^2. *E*'s reproductions also overlapped in several instances. In those observers, however, who had been most accurate in the identifying of notes, namely, *C, H* and *G,* no overlapping ever occurred; their 'found' notes represented relatively narrow bands, located at appropriate regions of the tonal scale.

IV. Summary and Conclusions

Our results show that memory for absolute pitch is not an 'all-or-none' capacity as has sometimes been supposed, but is a capacity whose degree of perfection is subject to variations and limitations of various sorts:

a. Individual variations. Certain individuals who possess this capacity are found to be exceedingly inaccurate in their identifications of pitch, even when the most favorable conditions are present; these individuals differ but slightly from normal capacity to identify pitch. In other individuals, however, the capacity is present in such a high degree of perfection that erroneous identifications are exceedingly rare; these individuals represent an extreme degree of supra-normality. Other possessors of the capacity are distributed more or less uniformly between these two extremes. Cases of absolute pitch memory may, therefore, be represented as a series of gradations which extends from normality at the one extreme to a high degree of deviation from normality at the other extreme.

b. Variation with clang-tint. Clang-tint proves to be an influential factor in determining the efficiency of absolute pitch memory. Pitches of certain clang-tints are relatively easy to identify, while pitches of other clang-tints prove to be difficult,—even impossible in the case of certain individuals. An observer whose percentage of error with certain clang-tints is less than 4 per cent may have a percentage of error amounting to nearly 40 per cent in other clang-tints. Piano notes are relatively easy to identify, while tuning-fork notes and notes sung by the human voice seem to stand at the opposite extreme.

c. The influence of tonal region. Notes chosen from the central region of the tonal scale are identified with a much higher degree of accuracy than very high notes or very low notes; an individual who is fairly successful in identifying notes of the once-accented octave may wholly fail in his attempts to identify notes selected from either end of the keyboard.

Errors in identifying pitches are of two wholly different sorts: Note-errors and octave-errors. In the former case the observer errs by a few semitones; in the latter case he names the note correctly but assigns it to the wrong octave. This illusion of the octave is of frequent occurrence in cases of absolute pitch memory. Note-errors vary in magnitude; but errors of small magnitude tend to predominate in proportion as the observer in question ranks high in efficiency.

Observers tend to be especially accurate in their identification of the pitch of certain notes of the octave, and to be especially inaccurate in their identification of the pitch of other notes. But there seems to be little agreement among observers as to which is the preferred note of the octave; and the note with which a given individual is most successful in her organ identifications may not be identical with her most successful note in piano identifications.

In experiments with higher pitches than are employed in music one finds that identification is still possible although accuracy is somewhat impaired. It is doubtful if in any previous experience of our observers these high pitches had ever been heard in conjunction with their note-names; and it seems remarkable that their identifications should have been accurate in such a large proportion of the cases. This phenomenon, together with the illusion of the octave, suggests that overtones may have played a prominent rôle throughout in the identifications of our observers; but although the physical composition of the sound-wave may be cited in support of this view, there are certain features of our findings which can not be brought into conformity with this hypothesis, —for instance, how are we to account for the fact that a note is much more likely to be confused with its third or its sixth than with its fourth or its fifth?

The fact that observers are much more successful through-

out in identifying the pitch of certain notes of the octave than in identifying others would seem to indicate that certain notes of the octave possess a distinctive and recognizable individuality. And this suggests an alternative explanation of absolute pitch memory. Recent writers have advocated the view that certain specific 'qualities' or 'characters' attach to certain specific regions of the tonal scale; that besides its pitch, its clang-tint and its other traditional attributes, the sound of middle c, for instance, has an attribute of c-ness which not only differentiates it from its fellows within the octave, but relates it with the c's of other octaves.[18] If this view be granted one finds a ready explanation for certain of our findings which otherwise prove to be mysterious and baffling.

When one seeks to determine what relationship exists between the ordinary process of recognizing, as it runs its course in the everyday experiences of normal individuals, and the process of recognizing pitches, as it runs its course in the exceptional individuals who served as subjects in our experiments, one finds that the known facts of the recognitive process support the following conception of this relationship. A comparison of one's consecutive recognitions of a recurrent stimulus reveals the fact that the ordinary process of

[18] Köhler has discovered that a specific 'quality' or 'vocality' (a *u* sound) attaches to a pitch of about 262 vibrations (pure tone, unmixed with overtones); that a second specific 'quality' or 'vocality' (an *o* sound) attaches to a pitch of about 522 vibrations; that a third (an *a* sound) attaches to a pitch of about 1055 vibrations; and that a fourth (an *e* sound) attaches to a pitch of about 2090 vibrations. (W. Köhler, Akustische Untersuchungen. *Zeitschrift für Psychologie,* LIV., 1910, pp. 241ff., and LVIII., 1911, pp. 59-140, especially p. 128; Ueber akustische Prinzipalqualitäten. *Bericht über den IV. Kongress für experimentelle Psychologie,* 1911, p. 231.) And Révész reports that even unmusical observers, who are unfamiliar with musical intervals, are able to detect a 'similarity' in the pitches of pure tones which are selected from points an octave apart on the tonal scale,—from which he concludes that the c's possess a specific and common 'character' of c-ness, the d's a specific d-ness, etc. (G. Révész, Nachweis dass in der sogenannten Tonhöhe zwei voneinander unabhängige Eigenschaften zu unterscheiden sind. *Nachrichten der königlichen Gesellschaft der Wissenschaften zu Göttingen,* Math.-physik. Klasse, 1912, pp. 247-252; *Tonpsychologie,* 1913, pp. 90-101.) See also J. D. Modell and G. J. Rich, A Preliminary Study of Vowel Qualities. *American Journal of Psychology,* XXVI., 1915, pp. 453-456.

recognizing has a typical genetic history and passes through a series of typical developmental stages. Especially in those cases of recognizing where one has to do with perplexing situations and where the process of recognizing is hesitant and difficult, the initial stages are characterized by a wealth and variety of mental content; and the subsequent stages are clearly distinguishable from one another. A difficult recognition is characterized at the outset by hesitancy and deliberation; it is the product of a process of discriminating, relating and comparing, and it is accomplished by means of imaginal and affective components which are definitely present to consciousness. But just in proportion as we become progressively more proficient in dealing with the recurrent situation, and as we 'learn to recognize' the datum in question, the affective and imaginal content becomes progressively more sparse and vague; and the process of recognizing becomes progressively and proportionately more prompt and facile.[19] There are of course hosts of experiences where, because of the simplicity and ease of the recognitive process involved or for other reasons, we pass directly and at once from the initial stage of development to the final stage; for instance, the normal individual recognizes reds and sours and warmths after but a single experience with them. The process of recognizing as delineated in the foregoing follows a general law of mental functioning which is illustrated, for instance, in every acquisition of mental or motor skill and in the formation of all mental and motor habits.

Now the process of recognizing pitches, as exemplified in our observers, bears a close resemblance to the final habituated stage of the ordinary recognitive process as exemplified in normal individuals. Every one is capable of recognizing promptly and accurately in certain domains; and one individual's efficiency of recognition in a given domain may be much superior to that of other individuals. The practiced student of geology never fails to recognize that a trilobite is a trilobite; and the efficient student of German invariably

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[19] For a detailed description of the genetic stages in the development of recognition see E. L. Woods, An Experimental Analysis of the Process of Recognizing. *American Journal of Psychology,* XXVI., 1915, pp. 313-387.

recognizes that this neuter noun is neuter, and that that masculine noun is masculine. Mr. A finds it possible to differentiate and recognize the twins in his neighbor's family, although he formerly failed to do so and although many of their acquaintances still fail. (The reason for his success lies in his discovery that Lottie has a tiny mole on her cheek, while Tottie has no such distinguishing mark; and immediately on the discovery of this obscure criterion his efficiency of recognition mounted from zero to nearly one hundred per cent.) Now the recognizing accomplished by the exceptional individuals who served in our experiments is, in its present perfect form, so similar in kind to the student's recognizing of trilobites that one might be tempted to assume that this final goal had been reached by the same general route and after the same toilsome journey in the two cases. But nothing could be farther from the truth. In the first place, such experimental evidence as the literature affords makes it seem very doubtful that absolute pitch memory can be acquired by training.[20] And in the second place, all of our observers report that their memory for absolute pitch is not a product of training; they simply discovered at an early age,—in some cases at the age of five years,—that they possessed this capacity, and possessed it apparently in quite as perfect form as at present.

In view of all of these circumstances, may we not therefore assume that memory for absolute pitch is based upon an ability to detect the presence of the c-quality which is obscurely present in every c, of the d-quality which is obscurely present in every d, etc.? And that given this basis upon which to build, the advent of our observers' capacity to recognize pitches was as inevitable and as abrupt as was the advent of Mr. A's capacity to recognize Lottie on discovering that she possessed a distinguishing mark?

[20] Max Meyer reports a series of experiments from which he seems to infer that memory for absolute pitch may be developed by training; but an examination of his findings leaves one in doubt as to whether any improvement actually took place during the progress of the training. Meyer's investigation throws no light on the question as to whether an individual who possesses no memory for absolute pitch can acquire that capacity by training. (Max Meyer, Is the Memory of Absolute Pitch Capable of Development by Training? *Psychological Review,* VI., 1899, pp. 514-516.)

SOME EXPERIMENTS ON THE CONSCIOUSNESS OF MEANING

By R. M. Ogden, Cornell University

I. Procedure

During the autumn of 1909 the writer began, in the psychological laboratory of the University of Tennessee, a series of experiments on the consciousness of meaning. The experiments were of the familiar 'reaction' type. The instruction, which was read to the observer at the beginning of each experimental period, ran as follows:

Close your eyes and hold yourself as passive as possible, both mentally and physically. After saying 'Ready,' 'Now,' I shall pronounce a word. Give it your immediate, full attention with the definite purpose of understanding its meaning. As soon as you are satisfied that the meaning has been grasped, react with 'Yes.' Then recount by introspection your complete experience. Be in no hurry to react, and let your occupation with the word be as natural as possible.

Among the observers who participated were Messrs. W. S. Adkins (*A*), W. E. Bryan (*B*) and F. M. Davis (*D*), all students of the university, the first two of the class of 1910, the third of the class of 1911; also Miss Sabra Vought (*V*), the university librarian. The observers worked separately with the experimenter in a quiet room. The duration of each sitting was approximately one hour, at various times of the day between nine and six o'clock.

The experiments whose results are here recorded form part of a comprehensive investigation which was continued over

a period of two years.[1] We shall limit our report to four series of experiments in which the meaning of words was the subject of investigation. In the first three of these series the words were presented orally by the experimenter, and the reaction-time was taken with a stop-watch. In the fourth series, begun in May 1910, and continuing into June, the words, printed by hand on cards with letters approximately 5 mm. in height and 5 mm. from center to center, were exposed in a card-changer. The reactions were recorded by a voice-key, and the time was measured by a Hipp chronoscope. The instruction for this last series was modified to read as follows:

Hold yourself as passive as possible during the fore-period. Direct your gaze towards the opening in the exposition apparatus. When the word appears give it your immediate, full attention with the definite purpose of understanding its meaning. As soon as you are satisfied that the meaning has been grasped, react with 'Peh,' explosively against the sound-contact, and close your eyes at the same time. Then recount introspectively your complete experience. Be in no hurry to react, and let your occupation with the word be as natural as possible.

In series I there were fifty-five concrete one-syllable words, such as *blade, dial, fan, grog, rat, pan, street,* and *van.* In series II there were fifty-five one-syllable words, mainly abstract: verbs, adjectives, prepositions and conjunctions, for which an object could not be readily represented, such as *ran, win, good, the, did, to, or,* etc. Series III consisted of abstract and concrete words in about equal proportion, together with a number repeated after varying intervals from the two previous series. The repetitions are not here reported. The full number of new words in this series was 60, but only two observers, *D* and *V,* completed the series. *A* performed half the number, and *B* did not participate.

[1] Previous publications of the author based upon this investigation are as follows: Knowing and Expressing, *Pedagogical Seminary,* XVIII., 1911, pp. 47-53; Content versus 'Kundgabe' in Introspection, *Journal of Philosophy, Psychology and Scientific Methods,* X., 1913, pp. 403-411; Experimental Criteria for Differentiating Memory and Imagination in Projected Visual Images, *Psychological Review,* XX., 1913, pp. 378-410.

In series IV, which extended over a period of about four weeks, *A* and *D* were the observers. The words used were 130 nouns selected in accordance with certain suggestions of Sommer.[2] There were thirteen classes of ten words each: eight referred to concrete objects and five to abstract conditions, processes and concepts. The classes with illustrative words follow:

I. Plants: *maple, lily, clover.*
II. Animals: *giraffe, elephant, donkey.*
III. Parts of the Body: *nose, lung, stomach.*
IV. House Furniture: *table, couch, picture.*
V. Inorganic Matter: *rock, iron, radium.*
VI. Designations of Family Relationship: *uncle, wife, son.*
VII. Vocational Designations: *lawyer, priest, grocer.*
VIII. Articles of Food: *steak, pie, coffee.*
IX. Conditions of Unpleasantness: *anger, sorrow, ennui.*
X. Conditions of Pleasantness: *joy, sympathy, comedy.*
XI. Conditions of Volitional Experience: *impulse, command, effort.*
XII. Processes of Intellectual Experience: *concept, attention, doubt.*
XIII. Social Conditions: *justice, law, economy.*

The words were given in a mixed order so planned that no two words of the same class followed each other. At irregular intervals words previously given in the series were repeated. No report is here made upon the repetitions.

None of our observers had previous training in introspection, but all had taken at least an introductory course in psychology. *A, B* and *D* were taking advanced work at the time, and had some general knowledge of the problem under investigation.

II. Reaction Time

A study of the reaction times gave the results indicated in the following tables. Those of the first series are not altogether reliable owing to an unsatisfactory watch used during part of the experiments. (All reaction times are recorded in seconds.)

[2] R. Sommer, *Lehrbuch der psychopathologischen Untersuchungs-Methoden.* Berlin, 1899, pp. 342f.

Series I

Obs.	No. of Exps.	*a*	*m*	*mv*	*Range*
A	48	6.82	5.8	2.23	3.4-20.4
D	45	1.56	1.4	.34	1.0- 3.4
V	43	2.07	2.0	.493	1.2- 3.8
B	46	1.8	1.8	.226	1.4- 3.0

(The records of the first sitting are omitted in this series.)

Series II

A	51	7.11	6.6	1.41	4.6 -12.6
D	53	1.18	1.2	.14	.8 - 1.8
V	51	1.67	1.6	.219	1.0 - 3.2
B	43	2.05	2.0	.412	1.2 - 3.6

Series III

A	30	5.74	5.3	1.393	3.2 -11.6
D	60	1.316	1.2	.208	.8 - 2.0
V	57	1.72	1.8	.215	1.0 - 2.8

Series IV

A	99	1.1214	1.062	.168	.695 -2.212
D	106	.8562	.7915	.183	.441 -1.989

a = arithmetic mean; *m* = median; *mv* = mean variation

The usable results were sometimes less than the total number of experiments performed. In Series IV many reactions were not recorded because the observer had failed to actuate the voice-key.

In practically every case the mean is greater than the median. We may regard the latter value as more typical since the higher value of the former rests upon certain exceptionally long reactions. *A* has the longest median reaction time in each series, and *D* the shortest. The significance of this is indicated by the introspections, and will be referred to later.

A required more time for the abstract words of Series II than for the concrete words of Series I, yet his *range* and *mv* decreased, indicating the effect of practice in standardizing his procedure. In Series III, which contained both abstract and concrete words, the lesser numbers may be attributed chiefly to practice. *D* reduced all his figures in Series II, and they remain approximately constant in Series III, though the *mv* of Series III is higher. A similar result is noted for *V* without increase in the *mv* of Series III. *B*, on the contrary, increased both *m*, *mv* and *range* in Series II. An

increase in difficulty of abstract words appears, therefore, in the cases of *A* and *B*, but is seemingly offset, if existent at all, in the cases of *D* and *V* by the effects of practice.

Series IV shows a striking reduction in the time of reaction for both the observers who participated, and especially for *A*. The average time is comparable with that ordinarily obtained by reaction to 'free' associations: it is within the range of 700σ to 1,400σ which is mentioned as typical by Myers.[3] This reduction in time seems to have been due partly to the technical superiority of the method: the visual presentation of the stimulus-word, voice-key reaction, and more immediate and accurate registration. Yet it also indicates a speeding up of the observer's activity, attributable in some degree, no doubt, to the urgent whirring of the clock. The instruction to "be in no hurry" appears, under these conditions, to be of negligible influence. As judged by his introspective reports, the shortened reaction time was in no wise disadvantageous to *D*. His reports were as full and complete as in the previous series. With *A*, however, there was often, though not always, a curtailment of thinking. In many of the experiments the report did not indicate that the problem of the word's meaning had been undertaken.

The influence upon the reaction of the type of word, as indicated by the thirteen classes previously mentioned, was found to be negligible. Report on this may therefore be omitted. A smaller *m* was obtained by both observers for concrete words (classes I-VIII) than for abstract words (classes IX-XIII): *A*, 1057σ for 63 concrete words, as against 1138.5σ for 36 abstract words; *D*, 792σ for 69 concrete words, as against 842σ for 38 abstract words. Similar results did not obtain for the *a*s. For *A* the *a* for abstract words (1126.8σ) was very slightly larger than that for concrete words (1118.8σ). For *D* the *a* for abstract words (780.96σ) was smaller than the *a* for concrete words (874.34σ). This rests, however, on the fact that four unusually long reactions chanced to fall among the concrete words. The median value is therefore the more significant.

[3] C. S. Myers, *A Text-Book of Experimental Psychology*, 1909, p. 134.

III. Statistical Analysis of Content

In the following table the contents reported in the first three series of experiments are given in percentage values for each observer:

	Concrete Imagery			Verbal Imagery			Imageless Contents		
Series......	I	II	III	I	II	III	I	II	III
Type[4]—	%	%	%	%	%	%	%	%	%
				A					
vis.......	47.2	30.1	44.0	5.4	41.5	16.0	....	....	....
aud......	....	....	6.0	5.4	15.0	4.0	....	....	....
kin.......	7.2	7.5	10.0	3.6	1.8	14.0	....	....	....
misc.	....	9.4	2.0	16.3	3.7	2.0	....	....	....
Total[4a].	49.0	41.5	58.0	30.7	58.4	36.0	63.3	86.7	86.0
				D					
vis.......	30.1	1.8	15.0	35.8	40.7	20.0	....	....	....
aud......	....	....	1.0	15.0	12.7	2.0	....	....	....
kin.......	3.7	3.6	5.0	49.0	5.5	5.0	....	....	....
misc.....	....	....	3.0	1.8	3.7	29.0	....	....	....
Total ..	32.0	5.4	23.0	58.4	59.2	53.0	96.2	94.4	83.0
				V					
vis.......	71.6	14.8	38.0	16.9	42.5	14.0	....	....	....
aud......	1.8	....	1.0	1.8	1.8	2.0	....	....	....
kin.......	....	1.8	6.0	....	..	1.0	....	....	....
misc.	....	9.2	....	3.7	7.2	9.0	....	....	....
Total ..	73.4	25.8	43.0	32.4	50.0	24.0	54.7	94.4	79.0
				B					
vis.......	90.9	41.8	....	1.8	37.2	....	....	....	....
aud......	....	....	....	....	9.3	....	....	....	....
kin.......	....	4.6	....	....	11.6	....	....	....	....
misc.	3.6	6.9	....	....	2.3	....	....	....	....
Total ..	90.9	51.1	....	1.8	51.1	....	32.7	79.0	....

[4] Types: Visual, auditory, kinaesthetic, miscellaneous and indeterminate; the last two classes may have included sensory data.

[4a] The "totals" are not simple additions, since the complete content of any one experiment was frequently a mixture of different qualitative ingredients. The percentages were determined by the number of experiments in which the various contents appeared, divided by the total number of experiments performed.

Visual imagery was generally the more frequent type, but shows a marked falling off in Series II where abstract words were used. The total concrete imagery shows a like falling off in Series II, and a recovery in Series III. A study of the words that occasioned concrete imagery in Series III shows that they were, indeed, the concrete words. For *A* but 7 of

the 29 words were abstract; for *D*, but 2 of the 24; and for *V*, but 12 of the 45. The verbal imagery was chiefly of visual type. It shows a marked increase in Series II for all observers save *D*. Imageless contents predominated in each series for *A* and *D*. For *V* and *B* they were less frequent in Series I, but more so in the other series.

In Series IV imagery was so negligible a factor that I have not thought it worthy of tabulation. The shortened reaction appears to have occasioned a dearth of associative contents, which appeared to be chiefly of imaginal quality. However, the stimulus word itself often persisted as a verbal image after the observer's eyes were closed. The typical report of *A* indicates first a conscious inspection of the word: a noting of its general size and form, peculiarities in printing, etc. (98 experiments, 75.3%). This was followed by, or occurred simultaneously with, pronunciation of the word, which seemed to be largely kinaesthetic (69 experiments, 53%). Then came an imageless notion of the meaning, and the reaction. Associative imagery was rare. Concrete visual imagery occurred but 10 times (7.6%), and a concrete kinaesthesis, apparently expressive of the word's meaning, was indicated but once. Verbal imagery, aside from that of pronouncing the word, occurred 8 times, twice visually, three times auditorily and three times in indefinite form. In addition, associated words were pronounced in 21 experiments.

In 88 experiments (67.6%) imageless contents were reported. Deducting six cases in which concrete visual imagery also appeared, seven in which the meaning of the word was associated with an object present to the senses, and two in which there was definite reference to the direction of an object and therefore a possible kinaesthesis, we have remaining 73 experiments (56.1%) in which no imagery was detected by the observer. In four additional instances the meaning was reported as 'known but undeveloped.' These indicate something more than a mere reaction to 'familiarity' by their reference to the domain of thought in which the word belongs,[5]

[5] *Cf.* Messer's "Sphärenbewusstsein"—A. Messer, Experimentell-psychologische Untersuchungen über das Denken. *Archiv für die gesamte Psychologie*, VIII., 1906, p. 77.

or some definite statement as to knowledge about, or an ability to formulate knowledge concerning, the word. For instance:

IV. 78. *concept,* 1405σ . . . Recognized word, and just before reaction, some effort to make it definite. Did not succeed; knew what it meant,—some reference to psychology.

The general attitude of this observer was passive and phlegmatic in this series. He did not always secure a complete meaning. In 44.9% of the reports no particular meaning was evidenced as having been present in the main period.[6] The fact that a meaning was or was not secured did not affect the reaction time greatly. In 52 experiments in which a meaning was secured the *a* is 1164.5σ, and the *m* 1073σ. In 47 experiments in which a meaning was not secured the *a* is 1073.7σ and the *m* 1053σ. While the reactions without meaning were somewhat shorter, the two medians are not far apart.

D showed no such failure to do the required mental work under the conditions of the timing apparatus. With only five or six words of the entire series was a meaning lacking. These were cases in which the observer failed to grasp or could not understand the word, together with some in which an associative train monopolized the experience at the expense of a definite meaning. Verbal images and kinaesthesis were noted among the contents, but there was no instance of concrete auditory or visual imagery. Only 13 experiments (10%) were without definite imageless contents. Further detail as to the introspective reports is reserved for the next section.

No reference is made to affective contents. Specific reports

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[6] "Complete" and "incomplete" meanings are, of course, relative. The experimenter's judgment in the above differentiation was based upon the manifest content of the report. Though an interpretation of the word was not reported as present in the main period, it often developed in the after period, indicating that its foundation had previously been laid. The following case illustrates such a "meaningless" experience:

IV. 16. *picture,* —. Saw the card come into place. Some thought before that it would do this. Took in the word at a glance. Quickly looked at each letter from beginning to end. After that seemed mostly aware of sensations. Knew I had moved my head slightly to the right. Was distracted by the reaction. *After it came the thought: this exposure might be regarded as a picture.*

of 'pleasantness' and 'unpleasantness' were rare. Other and much more numerous references to 'feeling' are omitted because of their ambiguity and a lack of space for their adequate treatment.

IV. General Analysis of Introspective Reports

In the analysis of the introspective reports which follows, it will be necessary to confine ourselves to a selection which may characterize the mental processes of the different observers and afford some ground for a theory of meaning. Lack of space prevents a systematic treatment of the data. Some of the facts are therefore less well established than I could wish, and the theoretical considerations must accordingly remain tentative. Since *B* participated in but two of the series, and *V* in but three, I shall consider them first, reserving the more mature and elaborate results of *A* and *D* for the last.

(*A*). *The Introspections of B.* Generally *B* interpreted the instruction of Series I as a demand for a visual image. When such an image occurred, and was found to be sufficiently representative, the conditions of reaction were regarded as satisfied.

I. 15 *quilt*, 1.8″. Visual image of a quilt.—Doubt after the reaction regarding the word.—It was no special quilt, but was spread upon a bed (visual). Feeling that this was the meaning of the word; an adequate representative of the class. *S.I.*[7]: No definite indication that the satisfaction was *felt;* it was more like a thought.

Two moments are here noted: the image, and a judgment that the image was satisfactory. The *quilt* and *bed* seem to have been related.

Sometimes the meaning appeared to precede the image:

I. 38 *maid*, 2.2″. Housemaid was the meaning. Visual image of woman with broom doing work about the house; dressed as maids are usually dressed, but can't describe the dress. General impression of youth. *S.I.:* The meaning seemed to precede the image. It was a white maid; no familiarity. . .

[7] The S. I.," or secondary introspection, was supplemental and evoked by questioning of the experimenter. The questions were directed mainly upon the existence of images, inner speech and kinaesthesis.

I. 47 *well,* 1.4″. First a general impression of something dug in the ground, then a visual image of a well at home. Visual image of the back yard, also feeling of familiarity and a pleasant feeling as if at home. *S.I.:* After reaction came association of lumber in the yard. Saw a vague visual indication of the lumber in the main period, but it was then undeveloped and meaningless.

The judgment of meaning may be indicated before or after the appearance of the image. In both the above cases the general meaning of the word appeared first, but an image preceded the meaning of *lumber* which was not developed until after the reaction.

The above cases indicate that an image may arise with apparent spontaneity to be afterwards judged as representative; or it may be preceded by a generalized 'meaning' that is not further analysed. The nature of the *nucleus,* as we shall term the dispositional tendency that forms upon the presentation of the stimulus word to condition the later contents, is more clearly revealed in certain reports of Series II. As indicated in the results previously tabulated, concrete imagery played a smaller part in this series of abstract words than it did in the first with concrete words.

II. 23 *felt,* 1.8″. First the material from which hats are made; visual image of a hat made from this substance. Second, a boy in the university named *Felts;* a visual image of him,—this seemed peculiar,—a tendency to laugh. Third, the verb meaning with a tendency to reach out and grasp something. *S.I.:* There was no reference to past tense in the last thought. In the second there was a thought of the difference in spelling, then an image of the name, written, but not very vivid.

This experience was evidently more associative than interpretative. Except for the indication of a general meaning for *felt,* preceding the image of *hat,* and the meaning *feel* illustrated by the tendency to reach out and grasp, we have no strict control such as may be exerted by a well-defined nucleus. The irrelevancy of the second association is perhaps indicated in that it "seemed peculiar." The control of the nucleus was not sufficient to inhibit the association, yet the difference in spelling was noted.

A more definite control is manifest in the following:

II. 14 *old,* 2.4″. Familiarity of the word first noted; second, thought of as connected with age, no visual image noted; then, third, thought of as an adjective. It seemed I had gotten about all there was to get, and I reacted. *S.I.*: There was no pleasantness or unpleasantness connected with the familiarity. The second and third meanings were introduced by the thought of looking further into the word.

The moment of 'familiarity' here indicates the formation of the nucleus from which the two *notions*[7a] developed to give precise meanings.

In the following instance a mistaken interpretation dominates the experience, yet it is known to be mistaken, and the proper interpretation asserts itself as a parallel process:

II. 33 *June,* 2″. Thought of *Jew* during the pronunciation; then of some person, not definite, which has since worked out into a particular Jewess. There was no good meaning for *June,* though I felt familiar with the word and it was associated rather vaguely with a season of the year.

The two nuclei were present without apparent conflict, though neither developed completely within the main period.

The last case we shall cite of this observer shows the reaction to an inadequate image:

II. 11 *fell,* 1.4″. In the act of falling, and in past tense. It seems now to have been a visual image of a body in the act of falling which preceded the thought of the word in the past tense. I saw the body falling and then thought of it as not being past tense which was the thing wanted. *S.I.*: The visual image was not essential to the meaning, but perhaps it was to the meaning of past tense.

Here the image seems to be clearly illustrative. Its inadequacy was detected because it represented *falling,* not *fell.* This is an evidence of nuclear control.

Summarizing briefly, we may say that the instruction leads to the formation of a mental state upon the presentation of the stimulus word. This mental state, the nucleus, is the primary dispositional reaction to the word. It may evidence itself in consciousness as a generalized meaning, as more precise meaning contents of an imageless nature, or as a mere

[7a] For a definition of *notion* cf., the author's *Introduction to General Psychology,* 1914, pp. 90 ff.

attitude of familiarity. It may also be quite unconscious, so far as introspection can show, and yet evidence itself in its control over the associated imagery and notions of meaning that subsequently appear. It is this relational control which constitutes the meaning of the word.

(*B*). *The Introspections of V.* In Series I, *V* indicated a similar tendency to secure a visual image as the characteristic end-content of the reaction. This was often vivid and in 21 experiments formed a satisfactory content for the experience. In cases where the image did not appear spontaneously, or where a further thought developed upon it, imageless notions would arise. Sometimes these were felt to be inadequate, as in the following instance:

I.20 *day*, 3.8″. Could not get a clear image. Everything was yellowish brown; it might have been today or tomorrow. I could not see a *day*. Uncertain how to react. *S. I.*: The idea of looking out on a bright day was present, but seemed inadequate. There was a reference to present, past and future.

The central directing nucleus referred to *time*. The vague visualization of color and 'looking out' seemed unsatisfactory representations. The tendency to secure an image was so strong that the meaning, though present, was not focalized.

In other cases imageless notions were quite satisfactory.

I.52 *snake*, 1.2″. First a vague thought reference to what a snake is, then a reference to a picture of a python seen this afternoon in a book. There were no images here, but a slight feeling of revulsion. *S. I.*: The reference was no more realistic because of the feeling; it was merely to the picture seen.

The secondary introspection is of interest in its suggestion that the meaning was not borne by the feeling of revulsion as a vehicle.

In some cases the thought reference was clearly in mind before the image appeared:

I.23 *keel*, 2.2″. No very clear image of anything. First the idea of a *keel* of a ship: things were a dark blue. After that a vague outline of the *keel* of the "Half Moon" [associated by the, then, recent Hudson Celebration]. I really saw the *prow*, not the *keel*. *S. I.*: The first idea was probably a mere thought of the connection of the word with a ship; this became explicit when I saw the prow of the "Half Moon."

The general 'idea' of meaning arising from the nucleus was here attended by a color to constitute a sort of *Bewusstseinslage.*

The meaning may develop from a verbal image:

I.27 *seed,* 3.6″. Picture of the word *seed.* General idea of what a *seed* is; no particular *seed,* but as a thing from which things grow. Consciousness that the word was understood. *S.I.:* A slight hesitation as though waiting for something more definite after the idea. The thought of seed time was in mind. There was a tendency to get a more definite image which did not come.

We see here the unconscious influence of the habituated task to get a definite concrete image, yet the verbal image served quite well as a point of departure for the meaning.

A definite meaning with an indefinite reference is indicated in the following case:

I.31 *freak,* 2.6″. Simply a feeling that I knew what it was: a vague idea that it was the sort of thing one sees at a museum or circus. No definite picture of a particular *freak.* Conscious of the unusual. *S.I.:* No particular demand for an image. Fact that image was lacking caused no disturbance. Conscious that I could have recalled some kind of *freak* seen in the past. Human peculiarity was the meaning, probably rather limited by reference to circus, though I have never seen a circus 'side-show.' I might have thought of exhibits at the Surgeon General's Office, but did not.

Though this meaning appears as definite and satisfactory as need be, and prescribes the sphere of reference, no precise experience of the past was suggested.

I.48 *toy,* 1.6″. General idea of meaning: something for a child to play with. Vague idea of a toy shop. No pictures. *S.I.:* No other images. Personal attitude not noted, but rather passive.

Here the meaning content arises clearly from the nuclear setting and is then exemplified. The former, 'something-for-a-child-to-play-with,' is a typical *notion, i. e.,* an elemental thought content. It embodies the meaning much more completely than would a concrete reference or image. As to the possibility that the observer may have experienced the words of her report in the main period, we have only her statement that this was not the case. Yet the presence of words in the main period is not difficult to detect, as is indicated by

the fact that this observer reported the presence of words in nearly one-third of the experiments of this series.

That words themselves may be conscious in imageless terms is suggested by several reports of this observer:

I.50 *plain,* 2.2″. First *plain* then *plane;* no image of either. The second has something to do with geometry, this fact was recognized. Nothing particular in connection with the first. No image of anything. *S.I.*: *Plain* differed from *plane* in spelling, whether thought or seen I do not know, but the *i* in *plain* was not noted as conspicuous. The difference in meaning was noted as a difference in spelling, but not developed.

The chief content here concerns the difference in spelling of these two words, yet the most striking feature of this difference, the *i* in *plain,* was not conspicuous although the only verbal imagery admitted as possible was of visual order. In Series III there were eight cases in which a consciousness of spelling was reported without verbal imagery. I may cite the following instance:

III.75 *waist,* 1.8″. Part of a woman's dress, the particular part called *waist;* then *waste,* meaning wastefulness. *S. I.:* Conscious of different spellings; the two spellings were present at the end as thoughts, not images.

Several cases from Series II indicate the concreteness of notions to which no imagery was attached.

II.36 *be,* 1.8″. *Bee:* no image of the word, but thought of what it meant, and an intention to think of a man I know who is a bee specialist. No thought of the verb. *S.I.*: No images at all.

The evidence for imageless contents is strong here. Knowing that this observer visualizes with ease, it is hard to believe that she could have overlooked such images as those of *bee* and the person referred to, had they been present. Another case shows a similar lack of imagery where imagery might have been expected:

II.41 *law,* 1.6″. Thought it had something to do with courts: then thought of the Law School and of Judge T——. The word had to do with legal phases of life. *S.I.*: There were no images.

The meaning was clearly abstract, yet the accessory associations were both concrete and image-provoking.

II.39 *than,* 1.8″. Recognized the word as known; a feeling

of contrast. Knew it was a word used to mean contrast between words. *S.I.*: The contrast reference was grammatical rather than real.

In this instance we have the nuclear meaning expressed as a 'feeling of contrast.' Yet there was no relating of contrasting words. Only the notion of the grammatical meaning of the word arose from the nucleus.

V usually responded by simply relating the word as heard to an image or notion which arose from the nucleus. The nucleus was commonly unconscious, though its existence was sometimes indicated by 'feelings of recognition' or 'knowledge,' and also occasionally by vague color experiences, such as the following:

I.8 *street:* "indefinite grey image; was trying to find something more precise."
I.10 *stalk:* (heard as stork) "a black experience."
I.17 *tube:* "a brown haze, uncertain as to the word."
I.20 *day:* "yellowish brown."
I.23 *keel:* "a dark blue."

The definite act of relating the terms,—the stimulus word, the nucleus, and the end-term, image or notion,—was not often evidenced in the introspection.

(*C*). *The Introspections of A.* With *A* images played a more modest rôle than with *B* or *V*. Although frequent in Series I, they were usually vague and inadequate as compared with the more definite notional contents. Sometimes the meaning would 'fringe' upon an image as in the following case:

I.34 *cross, 7.8″*. Visual image: vague, square, cross, long vertical and short cross-piece: brief reference to crucifix and Christ; then thought of Maltese Cross, vague visual image. No new meaning; hesitation and reaction. *S.I.:* Cross seemed heavy and large for use in crucifixion. The reference was to the definite event, not to rosary, or crucifix used by Catholics. 'Maltese' may or may not have been present as a word. No religious significance to the experience.

The criticism of the first image as inadequate to the meaning of crucifixion is of interest as distinguishing the meaning and the image.

More often the thought was given without an attendant image, though sometimes a rudimentary sort of image would develop from the meaning, as in the following case:

I32. *cab,* 8.6″. Only one meaning: something on the street; then details came; driver's seat, general impression of shape and size. No very clear image, but feel sure I could have made an image if necessary. Reaction when no clearer details seemed to come. *S. I.:* Can not say if the *cab* had two or four wheels, one or two horses, but am sure as to meaning. Size and shape,—no comparisons; shape, horizontal, oblong; size, a negative judgment, possibly, *i. e.,* about the size of ordinary cabs. More actively engaged; tension felt.

The original meaning appears to have been very general. There is a possibility of kinaesthesis in connection with the details of size and shape, yet these were quite subsidiary to the first impression of meaning.

The following case indicates imageless contents developing associatively:

I.22 *tile,* 5.8″. First thought of it as something red; then as something to make a roof of; finally, of a particular house covered with *tile.* The last reference was not full, and was concerned mostly with the roof. No visual image. Reaction. *S. I.:* No image to the first (*red*), just a meaning. No image to the second thought, no motor tendencies or feeling of extension. The last reference was to the T— house. No visual image. A reference to the shape of the roof, gables, etc., was also a part of the experience, but nothing definite regarding the house.

It is evident that the first and last moments could have been adequately visualized. That they were not, suggests that such an experience as 'redness' may be thought without being seen.

I.17 *tube,* 5.8″. A moment's hesitancy over the pronunciation. Idea of test-tube fixed upon, also idea of weighing in a chemical balance. No visual images noted. After the reaction, a visual image of a tube with a liquid in it. *S. I.:* Ideas of test-tube and weighing seemed of about equal importance. No tendency to evaluate either. Hesitancy involved in doubt as to word: *two* or *tube?*

The fact that the weighing idea is accessory, and relatively remote, together with the secondary report that the two ideas were of equal importance, leads us to conclude that the general sphere of chemistry constituted the nucleus of this meaning, and determined the two notions associatively. This nucleus may have been quite unconscious. As to the content

of the two 'ideas,' had the weighing been visualized it must have been circumstantial enough for recall. The test-tube, if originally imaged, must have been incipient since an image of it developed after the reaction. It should be noted that no indefiniteness of meaning need attach to such incipiency. Attempts have been made to trace 'imageless' contents to fragmentary imagery.[8] Here we seem to have the meaning first and the image developing later. The image appears to function as an *expression, illustration* or *example* of a meaning that may be quite complete and definite before any image has developed.

From Series II we shall report three cases. In the first two visual imagery is conspicuous, yet the relational function is clearly the important constituent of the meaning.

II.7 *in*, 6″. Relatively easy. Accompanied by a series of images. No attempt, as before, to get the abstract meaning. First, seemed to be in a room with an idea of the door and outside. Reference to door, left; no special kinaesthetic tendencies or sensations. Second, disturbed by noise of locomotive; associated inside the cab of the engine, or perhaps inside the boiler; vague visual image, maybe. Hesitation preceded the reaction. *S. I.:* I seemed to be outside this room; thought reference to the inside of the lecture room [adjacent] which was visualized. The second was also a personal experience: looking into the engine from some point on the tender. . .

While visual images were conspicuous contents, they were evidently illustrative and expressive rather than essential constituents of the meaning. In both cases this involved the relation of the observer, from a place where he seemed to be, to a place where he was not. Inadequacy of definite images to satisfy the nuclear requirements is indicated in the following:

II.34 *three*, 6.2″. Visual image of a large figure *three* in front. Could see double outlines, and light shading between them. Then followed the same *three* with a large *one* run

[8] *Cf.* W. F. Book, On the Genesis and Development of Conscious Attitudes, *Psychological Review,* XVII., 1910, pp. 381-398; H. Kakise, A Preliminary Experimental Study of the Conscious Concomitants of Understanding, *American Journal of Psychology,* XXII., 1911, pp. 14-64; M. F. Washburn, The Physiological Basis of Relational Processes, *Psychological Bulletin,* VI., 1909, pp. 369-378.

through it: "3 in 1" as the name of a liniment or oil, not definite. Went on to the meaning. Hard to get a general meaning. Three fingers of my right hand, forefinger and two others vaguely visualized. Attention seemed directed downwards to the hand, yet I think there was no sensation from the hand. *S.I.:* There was no thought of the Trinity. The picture referred to the label of a bottle.

The observer did not regard his first concrete reference and image as satisfying the demand for meaning, and proceeded to a more general interpretation which again led to a concrete reference, three fingers. Apparently it is the functional relationship that counts in meaning, not the content whether imaged or imageless.

The next case shows elaborate associative processes attaching to a proper name:

II.40 *Tom,* 6.8″. (1) Between 'Now' and the presentation of the word I heard the old "maple-sugar man" outside. (2) Understood the word as *Tom,* and recognized it at once as a man's name. (3) Reference or intention towards a past experiment of this sort, the particular name not recalled. (4) This brought me back to the old man; wondered if his name were *Tom,* did not seem likely and was rejected. (5) Thought of *tom* as a cat: name of a male cat. (6) That associated something from Elbert Hubbard about a cat, a reference to the situation, he called it a *tom.* This was known, not imaged. (7) General meaning then of *Tom* as a name. *S.I.:* The last phase (7) was more developed than the second. There was, however, no other reference to a particular person, nor thought of *Tom* as an abbreviation of Thomas. The fourth phase was rather passive, the sixth slightly funny. I did not have the whole story before me, but there was a reference to it as if I knew to what portion 'the tom' belonged and could, if I wished, reconstruct the rest of the tale. The third phase referred to a past experience in which the "maple-sugar man" had figured.

The two meanings of this experience, the name of a man, and the designation of a male cat, dominate despite the freedom of associative references. It is of interest that the last phase should recur to the original meaning.

That associative tendencies are not always subject to the control of functional relevancy is suggested in the following case from Series III:

III.6 *poet,* 6.2″. The word was visualized, then the name

Shakespeare, possibly visualized, then *Dichter, Goethe,* possibly visualized. Tried to get the meaning of *poet.* Visual image of certain printed lines of verse, rather long. Reacted. *S.I.:* No particular recognized verses.

The contents appear as a succession of verbal images, all associated, yet without a definite meaningful relation being established among them. The objective relevancy of *Shakespeare, Dichter, Goethe* is obvious. The image of verses is less so, for it does not carry out the explicit tendency to get the meaning of *poet.* Association seems to have had the upper hand. Though conscious of the problem, the observer did not definitely proceed to establish those relations among the associated contents which would have given a precise meaning to the word.

The nucleus may classify the word very simply without the aid of images, as in the following case:

III.19 *warn,* 4.6″. Pronunciation seemed a little drawn out. Word seemed hard to define, though I recognized it. Simply thought of warning not to do. Nothing definite or specific; no person or action. *S.I.:* No images.

The abstract notion 'not to do' as related to *warn* establishes the meaning. It might have been verbalized, yet the observer reports no images.

III.36 *grown,* 3.2″. No consciousness of spelling, only one meaning, that of a grown-up person. No image. Thought reference to children as in contrast. *S.I.:* It seemed somehow to have the force of an adverb, instead of an adjective. Feeling of peculiarity.

The word itself again offers the key to the nucleus: a *grown* or *growing* person. The adverbial significance is interesting, as it appears to establish a subtler relationship with children, not yet *grown* but also *growing.*

III.43 *phlox,* 4.4″. Could not quite make out the word at first, though I had perceived it clearly. First meaning, *flocks* of animals, can't say what kind. Vague consciousness of spelling, though nothing seemed visualized. Second, thought that it also means a flower. Was conscious that the spelling was different, but did not think specifically of this. Thought the flower was pink, no image (I know now that it is not necessarily pink). *S.I.:* . . . the first reference could have been to birds, not definite.

This report is interesting in several ways. First, a 'clear perception' is not necessarily attended by meaning; a second content must first be present, and a relationship established. This came with the reference to *flocks,* which was definite, though general, since it might have been birds or animals. A second content *flower* also is established in meaning relation, together with its association of *pink.* The difference in spelling was probably but a vague awareness attaching to the two notions in their respective relations with the word. The first spelling may have been definitely known; the second was probably known only as being different from the first.

A final selection from this series gives evidence of the span of thought which may be involved in a single thought process:

III.46 *pair,* 5.6″. I was waiting for the word. When it came it associated almost immediately two meanings: *pair,* as a pair of something, boots or shoes, and *pear,* briefly, not visualized. I came back to *pair* which was probably present all the time. This experience was very active and pleasant. I resumed in a very brief thought a problem in Poincaré's book having to do with a certain theory in logic. Lingered over the thought for a moment while some imagery developed, possibly a vague image of a printed page and the peculiar type of the French,—not visualized, I think. There was something about the appearance of the numbers,—many numbers are scattered through the book. With the reaction came a visual image of a *pear.* It seemed floating in the air. I could see part of the stem, like tobacco smoke, vibrating. *S.I.:* Content of the thought from Poincaré: If there were an infinite number of pairs of boots extended in a line, the number of boots would be twice the number of pairs; still you could not consider the number of boots to be greater than the number of pairs, since both are infinite. This argument was to refute Zermelo's axiom,—not definitely conscious what this is. Also a thought of Russell who proposed the problem and solved it, and that Poincaré agrees.

The general structure of this experience is like the preceding one. After establishing two meanings, the thought runs from one to the other, associating and establishing supplemental relations with each. The first reference to *pair* proved the more interesting and thought provoking, yet the experiment terminates with a return to *pear,* and a visual image. *Pair* at once reproduced a thought recently in mind, and apparently

quite compact with meaning. The analysis of this content as given in the secondary report is more largely informative than descriptive of content. Yet we need not doubt but that knowledge of the problem with its differentiated relations to 'Zermelo's axiom,' to Russell and to Poincaré were in some sense present. The compact notional content is thrown into striking relief by contrast with the fragmentary imagery of the printed page. While this imagery may have significance as the expressive moment of the experience, its content can hardly be regarded as a satisfactory vehicle for the thought which preceded it.

Of the introspective reports in Series IV we have already noted that the new method with its consequent reduction in time of reaction led to a marked decrease in contents of experience as well as in the observer's active endeavor to secure meanings. The following case reveals the weakened influence of the instruction, though the associative content was not curtailed:

IV.124 *willow,* 1025σ. Pronounced. Noticed openness of the last *w* at the top. Rather a familiar word, and at the end two recollections. First, badly worked out, intended to refer to a certain short story where a good-natured fellow had been praising his own tobacco. A friend remarked that it was probably grown on willow trees near Magdeburg. The second referred to the Erlking. The word *Weide* distinctly pronounced. *S.I.:* It was a German story, but did not know if it was in German or English; have read it in both languages. The name Erlking was not present.

In the report of 'familiarity' we have an indication of the nucleus, yet it did not operate to give an interpretation of the word. The first association appears to have been imageless as to content, and quite specific as to intention. The reference to the "Erlkönig" and the pronunciation of *Weide* is an interesting irrelevancy. *Weide* appears once in the fourth line, sixth stanza, of Goethe's well-known poem: "Es scheint die alten Weiden so grau." Another instance showing lack of directive control is the following:

IV.125 *love,* 1154σ. Looked at the word absently at first, then pronounced lightly, noticing its shortness. An effort to recollect the name of a woman. Knew this to be her middle

name. Thought I knew two women whose middle name was *Love*. Confusing; casting about for quite a while. After reaction made out the name of one, the first, and spoke it: "Lula Love E—." No particular thought of her or image. Did not recollect the second. *S.I.:* No application of the meaning of the word to these persons.

This experience describes a thought process, though it leaves entirely aside the interpretation of the word. The 'free' association of the word as name of a person dominates, yet the conflict of two such persons with *Love* as a middle name is typical of an active process of thought.

In nineteen cases of this series (14.7%) a mere recognition or familiarity, together with an inspection of the word, made out the complete report. Among those experiments where a more definite meaning was evident, a large number consisted merely in the appearance of a relevant thought (27 cases, 20.9%). Usually the fact that this thought expresses a meaning and is therefore based upon an actual relation must be inferred, since the subjective act of connection or assimilation is not definitely reported. This may be illustrated by the following case:

IV.69 *farce*, 819σ. The lines of the letters appeared thin. The closed part of the *a* at the top was rather small. The letters were not well in line. Pronounced very soon after noting this peculiarity. Quite familiar. Reference to regular play-meaning. Nothing more specific; reacted quickly.

The defining reference to 'play-meaning' leaves no doubt that the meaning was secured. This is not a case of 'free' association, but an actual subsumption under an appropriate category.

The next three cases show an active participation based upon the arousal of specific interests:

IV.85 *crab*, 1075σ. Pronounced rather quickly. Noticed the last letters were higher than the first: *b* seemed large. Considerable interest in the word. Understood as a rather small animal something like a crayfish. After closing eyes a vague visual image of some such animal, more especially the main body. Some reference to zoological laboratory. Later, after reaction, some attempt at classification: 'crustacea' spoken.

The interest rests upon the zoological work in which this

observer was then primarily engaged, thus the experience was more intimate than many others.

IV.88 *lily,* 1326σ. At first glance noted the spelling. Meaning immediately that of a flower. Also a notion that there should be two *l*s. Looked at it again and saw that there were not two. Became conscious that it was also a girl's name. Then, after a little longer, that the two *l*s was the girl's name. The word for flower seemed to give some peculiar impression, rather delicate and pretty. No image of the flower or association. No feeling for the texture of a lily.

I have previously discussed this case in print[9] for the purpose of illustrating the distinction of knowing and expressing. The subjective act of interpretation is afforded by the conflict of the two meanings. It is noteworthy that the reference to spelling and the demand for two *l*s *preceded* the reference to a girl's name which justifies this demand. I rely upon this observer's careful method of introspection for my assurance as to the order in which these thoughts developed.

IV.110 *election,* 1255σ. Noticed the two *e*s; *c* not so tall as the other letters. Pronounced. Some indecision at first; groping for a meaning, got it as a political election,—no images. Was dissatisfied with that. Some time after the reaction there occurred the other meaning, that of *choice.* The word was spoken, and the first meaning recurred; also a tendency to refer to choice in election of studies in the university, not very definite.

We see here that the nucleus exercised a control, not merely to the extent of securing an adequate meaning content, but embracing a broader range than the obvious political election, and finally, though not until after the reaction, bringing this more general significance to consciousness.

These three cases indicate three special occasions for active participation in securing meanings: (1) a more than ordinary interest in the subject; (2) a conflict of interpretations, either fortuitous or predetermined by the nucleus; and (3) a demand for a further development than that afforded by an obvious associate.

In eighteen cases (13.9%) the meaning consisted in a reference to a general category or sphere of objects to which

[9] Knowing and Expressing, *Pedagogical Seminary,* XVIII., 1911, pp. 47-53.

the one suggested by the word belonged. It is not always easy to distinguish these clearly from the cases of mere recognition and 'familiarity,' yet reference to a superordiate class is often quite evident. For example:

IV.103 *artery*, ———. Saw the word. Referred almost immediately to zoological work: anatomy, no words. I seemed to know what it meant. Reacted without further thought. After reaction a vague image of a blood vessel taken, I suppose, from some book. *S.I.:* Not definite with reference to man or animal.

With five words: *sympathy, intention, attention, concept* and *memory,* there was but a general reference to the domain of psychology. References to objects or situations, present or recent, operated to give meaning in nine cases. Twice such references failed to give a meaning: with *nerve* a futile attempt was made to connect the word with the apparatus, and with *command* an equally futile attempt was made to connect it with something prior to the experiment.

In one instance alone imagery was a dominant factor in this series:

IV.105 *giraffe*, 1242σ. Pronounced. Peculiar appearance of the *i* between the *g* and *r*. Knew the giraffe lived in Africa. In latter part a distinct image of a group of giraffes seen at a distance in an open field. Some specialization of form, I think, in the same image. I made out the general shape of the body and long neck. *S.I.:* Image a representation from a moving picture. No thought that the image was moving, but some knowledge of the theater. No indications of color, even of the color of the projection which was brown.

The first reference to the animal as living in Africa appears to have been imageless. The noting of the animal's form may or may not have been contained in the subsequent image. Imageless specializations were frequent with this observer, as may be illustrated by the following cases:

IV.55 *chlorine*, 1315σ. Knew that it was a gas,—word not present; also some notion of its disagreeable character, not specific. No thought of odor or color. Some notion of it as a chemical substance,—vague.

IV.94 *sodium*, 952σ. . . . Word especially interesting; brought recollection of some word had this morning; now know it was *chlorine*. Thought of as being a metal, not very

vivid. Something peculiar, not definite what . . . *S.I.*: Substance is soft and springy-like, was the peculiarity.

In the latter we find a 'recollection' without the presence of the recollected object, and a 'peculiarity' without its nature being revealed until after the reaction.

The experiments of this series with *A* are especially instructive because of the opportunity afforded to compare reactions without meaning and those with meanings of varying degrees of completeness and definiteness. The results indicate that a special function is operative in securing the meaning, and that this function works upon a dispositional basis which we have termed a *nucleus*. It is evident that the reaction tendency, unduly excited, no doubt, by the special conditions of Series IV, and particularly by the whirring of the clock, became very urgent. The specific instructions to secure a meaning, and to "be in no hurry to react" were not usually followed as they had been in the previous series. If the nucleus formed readily and quickly, a meaning was grasped. Otherwise inspection of the word and 'free' association prevailed. It is worthy of note that the speeding up of this observer's reactions was not usually provocative of a tension conducive to thought. Carrying the extra load of distraction occasioned by the noise of the apparatus and the suggestion of a quick reaction did not result in focussing the observer's powers of thinking. He complied with the requirement of increased speed in reaction, but remained otherwise self-contained, phlegmatic and untouched by the demand for a meaning within the prescribed limit of the main period. Frequently, indeed, meanings were "intended" during the main period, yet were not developed, *i. e.*, the relations were not *established*,[10] until after the reaction had been made.

(*D*). *The Introspections of D.* Throughout the experiments *D* showed a tendency to play with the word at the beginning of the main period. In Series I, particularly, he usually began by noting the word and repeating it in visual, auditory and kinaesthetic terms. After this the meaning would come spontaneously, as a rule, with or without an attendant

[10] *Cf.* E. Westphal's discussion of stages of consciousness, *Archiv für die gesamte Psychologie*, XXI., 1911, pp. 229f.

image. During the time in which the observer was engaged with the verbal impression, the nucleus formed and, without an apparent conscious intermediary, occasioned the appearance of a content which by its relationship to the word afforded a meaning for it. These contents are of special interest because they were so often definite and concrete, although visual imagery was very infrequent. Some examples of rudimentary imagery are noted in the following cases:

I.12 *arch*, 2″. For a moment hesitation. Then experience as of something coming down on both sides and forming an arch, like a rocket bursting. Not visual. Then a visual image of an arch; not any one known; base not seen; was of brick, cement seen; color of bricks; pillars of stone with round ring capitals; not attached to a building. No hesitation in reaction. *S.I.:* First idea became the visual image. The first idea was not visual, but had location before me and at some distance.

Kinaesthetic factors, though not mentioned, are suggested by the description. Another instance of a similar process follows, though without visual supplementation:

I.19 *foot*, 1.4″. Moment of hesitation, then a tendency to form the shape of foot as by moving points: a small portion of the ankle. Then followed thought of one of my feet, the left, crossed. Although eyes were closed, they were directed towards it. The position of the foot was the most conspicuous feature of this experience. *S.I.:* The position of the formation of a foot was to the left in front, as usual. I was passive; it was formed for me,—no visual factors. Did not note any kinaesthetic sensations or images.

The next case indicates the presence of a concrete notion without imagery, and without the tracing process of the last two examples:

I.22 *tile*, 1.4″. First a visual image of the word in printed letters,—almost immediately. Next an idea of the shape of a tile such as is used in mantle construction. Finally a remembrance of a piece of plaster with which I had propped the door to this room. This was not a visual image. Reaction almost instantaneous. *S.I.:* The idea of the tile was not visual: size, 4″ by 6″; on two sides of back, close to edge, were ridges. There was no thought of the use of these. Location was at right of front, about at the corner of the room, half a dozen feet away. A tendency to turn when the recollection came to mind. [The piece of plaster reposed at the other corner opposite the observer.]

The 'idea' of the tile seems to have been solid, geometric, without evidence of imagery. This type of concrete notion is frequently met with in this observer's reports. Kinaesthesis, though not reported, is often suggested, yet it is difficult to reduce some of these contents to kinaesthetic terms. The following examples are typical:

I.21 *seed*, 1.8″. . . . Idea of a seed, 1/8 inch in diameter, with ridge running around it. Definite idea of something to plant and reproduce. No images.

I.32 *cab*, 1.2″. . . . Idea of a cab, vehicle to ride in; the sort you hire. *S.I.*: . . . No visual factors, but idea of bulky shape. It was a big four-wheeler, though the details were not given.

I.47 *well*, 1″. Kinaesthetic pronunciation of *w*, then *e-ll*, separated. Idea as if I were to say *well* as an exclamation. Then the noun meaning, being something to hold water, not distinguished from such a thing as a cistern. *S.I.:* It was definitely in the ground, no definite idea of depth, but more than ten feet, as though you looked down and could see so far without knowing how much farther. First the abstract notion, then a kinaesthetic attempt to look in well, not definitely located.

The notion of the meaning appears distinct from and antecedent to the kinaesthetic representation. The most striking case of an 'all-around view' obtained by this observer occurred in another series, not here reported, in which the instruction was to obtain a kinaesthetic image:

sinking, 2559σ. There was a well-defined sensation. For a second I got nothing, then a striking sensation of sinking. I once came near drowning; this was a recollection of that. It was as if I were really there. Almost the feel of the water, movement of limbs, etc.; the shape of the place I was in, and that I was in the middle of it. Almost a visual image, but only partially. It was quite complete. I don't think I saw it because some of the things which would have come in the line of vision were not there. It was not an image; I don't know why, but there were several other things about it. I knew just as well the things I could not have seen, as the things I could see; what was on both sides of a rock, what was behind me.

In this case the evident kinaesthesis differentiates itself sharply from the recollected situation with its simultaneous view of all sides. While imagery may have been involved in some

fragmentary way, it must have been inadequate for such a 'view' as is here described. I therefore regard this as essentially a notional representation.[11]

The meaning often preceded an imaginal representation as in the following case:

I.49 *smoke*, 1.2″. Kinaesthetic pronunciation and a visual image of *s-mo-ke*. In addition an idea of what smoke is. Then a visual image of smoke rising, not distinct, in front and rather high up. *S. I.:* The thought preceded the image. No thought of fire. A tendency to make an association with tobacco smoking was not made, but remained undeveloped.

A final case from this series describes concrete facts without imagery:

I.16 *bark*, 1.2″. Slight hesitation at first. Two associations: bark of tree, discarded; bark of dog, stronger and was followed by reaction. *S. I.:* Idea of size of dog, and something of color. My own dog, the way it acts, looks, etc. Various things associated with, and after, reaction. No visual images. I recollect details with reference to the animal, but can't image them. It is very difficult to get a visual image of the dog, only by an effort, after a pause. *Bark* associated my own dog with a decided feeling of pleasantness.

The details of size, color, looks and action of the dog are unfortunately not reported in the primary introspection. If we may rely upon the secondary report, they were of a sort which would be adequately expressed in visual or verbal terms. The fact that the observer could obtain a visual image of his dog only after an effort, suggests strongly that some other content was present in consciousness when the associations occurred in the main period.

In Series II *D* reported no case in which imagery was the principal content. Twenty reports revealed no imagery at

[11] Another interpretation is, however, possible. I regard *notions* as derived from conscious *relations*, as will be developed in the conclusion. If we assume here the presence of obscure though representative contents: imaginal and kinaesthetic, the meaning of the "view" might have been afforded by spatial and other relations established among these representative items. This would have involved a *reenactment* rather than a *review* of the scene. But the important factors of the experience would still be the imageless relations, not the obscure bits of magery and kinaesthesis. On the whole, an "all around" notional *view* seems to me a simpler explanation, but I shall not press the point.

all. Three cases are selected from this series for their intrinsic interest.

II.17 *gray*, 1.2″. With the pronunciation of *gra*, added kinaesthetically *ve*; an idea of a place of burial. The spelling given to the complete pronunciation was *grey*, meaning a color. No visual image, but there was an idea of what grey looks like. During the reaction, and immediately afterward there was some doubt if it should not be *gray*. *S.I.*: The difference between *grey* and *gray* was a thought, not an image. Certain that the meaning was the same in either case; but also, a little later, I thought of the name Gray.

There are three points of interest in this report: (1) The notion of what *grey* looks like, without visual imagery. This may be described as *potential knowledge*,[12] a readiness for apprehension, and a knowledge of ability to make a correct judgment; it is definitely intentional. (2) The thought of the alternative spellings *grey* and *gray*, without verbal imagery, although this observer reports such imagery frequently. (3) While the observer may not have known that the two spellings are permissible, his thought reaches a certain culmination in the post-reaction association with the name *Gray* which is usual only with the one spelling.

II.34 *three*, 1.2″. Visual image of the numeral *3*. Was so certain that I knew what this meant that I did not analyse it into the number of units. Before the reaction came a thought, not inwardly spoken: 'I certainly know what *3* is,' then I reacted. I really did not know what it was throughout.

This report suggests a consciousness of certainty without any real content of meaning. This is hardly a case of potential knowledge, since there is no indication of any intention to develop a meaning. The judgment appears to have been based upon an extrinsic familiarity.

II.37 *all*, 1.2″. *The whole* was the only meaning found. I attempted to find others. A printed visual image, *a w l* was present but there was no reference to its meaning. *S. I.*: *Awl* was not connected in any close way with the *all* meaning; it was just there as a visual image without meaning. The thought of attempting to find all the meanings possible had been in mind before the word was given. An attempt to find another meaning recurred at the end of the experiment.

We have here a curious disjunction of contents which might

[12] *Cf.* E. Westphal, *l. c.* p. 229.

have been related. The presence of *awl* was clearly associative, and its occurrence was probably in part due to the determining tendency formulated in the fore-period "to find all the meanings possible." Still it was not related to the task, although the observer recurred to this special auto-instruction before the reaction. This shows how dependent the meaning is upon an assimilative function made possible by the nucleus. The meaning of *all* dominated the experience, excluding *awl* because of its irrelevancy, despite the auto-instruction for more than one meaning.

From Series III I have selected three cases:

III.23 *alter,* 1.4″. Familiarity with the word. It associated the Catholic Church on Summit Hill with the idea of a boy reading from the pulpit, as seen the last time I was there. The boy was identified, though I remember no particular things about him. *S. I.:* No images. Could only have been one boy. Many of his characteristics were fresh in mind and came out immediately afterwards. He was an abominable reader.

There is no reference to the word in this report, yet the sphere of the association was clearly determined from the start by the nucleus of *altar.*

III.26 *myth,* 1.4″. First *myth;* some association with preceding word *muse.* Reference to History, and a reference to Greek. All this came before I had an idea what *myth* was; then only that it was legendary, not what it was about. *S. I.:* There was also a thought that *myth* and *muse* had been placed together for this purpose.

We find here that the presence of relevant associations does not constitute a meaning unless their relevancy is established; the contents must be related in a definite manner. The relevancy of History and Greek was not developed, for the meaning, *legendary,* clearly has its meaning in its general relation to *myth* and *muse.*

III.31 *pour,* 1″. First pore, meaning pore of the skin; then it meant *poor.* The fact that it is often pronounced 'pore,' and that this is incorrect, was also thought. *S. I.:* Conscious of the different spellings without images.

The consciousness of spelling without appropriate imagery, which was also present with *V* and *A,* was attested in 29 experiments of this series.

In Series IV inspection of the word when it appeared was frequently reported, though not so often as with *A*. A special note of the word was made in the reports 35 times (26.9%), whereas with *A* this occurred in 75.3% of the experiments. In fourteen of these cases with *D,* and in fourteen other experiments where inspection was not specially noted, the meaning of the word was derived with reference to the visual appearance of the letters. At times the meaning seemed little more than a nuclear reference, as in the following case:

IV.10 *execution,* —. Word referred immediately to such an execution as decapitation. Then, with reaction, tendencies towards other meanings, as the execution of any project. The word looked at in the first sense had a significance and weight of its own. I did not mean *decapitation,* but any kind of capital punishment, preferably *hanging.*

It should be noted that the nucleus determined the meaning in the looks of the word, not the opposite. A more elaborate case of a similar nature is the following:

IV.6 *law,* 1223σ. Noticed first that *law* was a smaller word; then that you might say it had an angular feeling, *l* and *w,* particularly, in the sense that *law* does not give way to anything. The word has a peculiar significance of its own. Some words give feelings of pleasure, some of displeasure; some enliven, some put us to sleep. *Law* is peculiar, it gives neither pleasure nor displeasure. If you took words of all sizes and shapes *law* would be like the top of this table, only longer, square cornered, inclusive. This is not all, but I don't know if I can say anything else. The meaning was really of civil law. Still it was taken in a broader sense. Laws which are badly made could not have been considered in this category. Laws of nature would not have been thought of, but if mentioned would have been admitted. The definition was, in short, the proper settlement of controversy in an absolute sense. At some time remembrance came that I am writing a speech on law. *S. I.:* . . . No images. In contrast to a small, rapid, erratic moving body, *law* would be a larger, slower, directly moving body with greater momentum, but less velocity. There was no feeling of movement when the word was experienced, but there was decidedly a feeling of force.

This account contains much that is informatory, along with its description of content. Many psychologists would be inclined to interpret the 'feelings' and representations here

noted as essential vehicles of the meaning process. Associations such as those described are no doubt frequently made by some persons, and thus become more or less permanent adjuncts upon the renewed experience of certain words. The observer's special interest in this word is attributable to the fact that he was engaged in writing a speech on law. It is thus possible that many of these characterizations had been considered previously. Yet the meaning, "the proper settlement of controversy in an absolute sense," is easily distinguishable from these associative and constructive characterizations of the word. The report notes four such characterizations: (1) the 'angular feeling' of the word, interpreted as meaning that 'law does not give way;' (2) the fact that the impression was neither pleasant nor unpleasant; (3) the impression of being flat, long and square cornered; (4) the impression of force. No one of these is altogether congruous with the meaning reported, and I would regard them all as more fortuitous than essential. The word gave rise to a nucleus which conditioned, aside from its meaning, several tendencies and functions. None of these was probably quite explicit. They are more of the order of potentialities than of actualities. While not unrelated to the central meaning, this meaning does not consist in them, but is rather the knowledge of a sphere of interest. The information regarding the tendencies felt is largely a matter of telling what would and what would not have fitted into the category which the nucleus embraced.

In other cases of this order, twenty-four in all, the tendency was more precisely to make the word look or sound like the object which it named. Sometimes this constituted the major content of the experience:

IV.20 *snail,* 1098σ. . . . Tendency to make the word sound and look like a snail. That is about all there was. *S. I.:* Repeated the word to myself. Resemblance as if the letters were changed to make the analogy. It just did that. Had an idea of size and shape of the animal. Quality of its sliminess also present. This attached itself to the letters.

The molding of the word into a resemblance of the object, rather than a spontaneous development of the object in the word, is here evident and is characteristic of most of these

analogies. Very few instances were reported where the appearance of the word might be said to determine the analogy before an intention of meaning developed. Sometimes, however, a suggestion lurking in the word's appearance may work out its resemblance irrespective of meaning:

IV.8 *vigor,* 1001σ. Simultaneously its meaning *strength* was associated with the word itself, which worked out into *vinegar.* The *vi* and *gor* separated in the printing.

One might suppose that the association with *vinegar* was mediated, or at least made possible, by the meaning *strength.* The secondary report does not support this:

The word *strength* was not present. It did not exactly mean that, but rather strength in action, as a significant quality among people.

This interpretation indicates no relation with *vinegar.* Another experiment bears witness to the independence of the meaning:

IV.29 *pillow,* —. Knew first that this was a support for the head. The letters have many perpendicular parts. They took the shape of a barrel-stave hammock. . . . *S. I.:* There was a vague connection between *pillow* and *hammock.* They had to agree before an analogy could be established.

The last sentence indicates the independence of 'barrel-stave hammock,' and the effort to make it relevant with a meaning already established. The shaping of the letters to form a resemblance to *hammock* was doubtless directed by an associative tendency. The meaning of *pillow* is not expressed by this, yet it forced the establishment of a relation with *hammock.*

It has been contended that the meaning of a thing is its 'context,'[13] *i. e.,* it consists in those contents, sensational or imaginal, which attend it and are associated with it in the present situation or by virtue of previously formed associations. If this were the case we should expect our instruction to be satisfied whenever appropriate imagery was recalled to form such a context. But associated contents appear to be few and negligible in this series, and when present they seldom shape the meaning, but are themselves shaped by the observ-

[13] *Cf.* E. B. Titchener, *A Text-Book of Psychology,* 1910, p. 367.

er's intention. It seems evident that the mental content, as content, must undergo some modification before it attains meaning. We have many instances where the necessary modification is lacking, and a meaning is therefore absent. But the modification in question is not a mere association of other contents. The meaning seems rather to be involved in a disposition which we have termed the nucleus. This forms as a result of determining tendencies that issue from the instruction and the stimulus word. These tendencies may be unconscious, or they may provoke 'feelings of familiarity,' 'recognition' and other vague and unanalysed *Bewusstseinslagen.* From the matrix of the nucleus arise the specific notions (*D* calls them 'ideas') and images which, when related to the nucleus or to the word as modified by its nuclear setting, register the knowledge which satisfies these determining tendencies. The knowledge may be slight or considerable, specific or general in scope. Feelings may occur to indicate the conformity or non-conformity of these hints to future expression with regard to the total situation. Further development is thus facilitated or inhibited, as the case may be. At the same time other processes and developments may accrue to the sense perception. The influence of the problem tends to brings these into relation with the nucleus, and for the most part we find them shaping themselves under this influence into movements and images of a congruous sort. Occasionally, however, they fail to conform and, as in the two last mentioned experiments, reveal the fact that they are not essential to the meaning.

D shows a marked contrast with *A* in the character of his meanings. There are no cases comparable to those of *A* in which he passively allowed the experiment to transpire without an effort to interpret the word. *D*'s meanings are also richer and more varied in content. Yet visual imagery was not reported once in this series, either of an object or of a word. Despite this, his thought was very concrete and played usually upon the same class of objects which most observers visualize. There is a possibility that unreported kinaesthesis, sensory or imaginal, plays a part in this observer's processes. Yet, unless we are disposed to adopt an elaborate

scheme of symbolic representation, such elements, even if present, would be inadequate to explain the nature of the contents described. A satisfactory congruity of kinaesthesis with the contents recorded is rare. The following experiment brings this out:

IV.27 *poplar,* 1697σ. A tendency was present to make *poplar* lengthen out and represent the height of a tree. Knew it was a tree before I fully realized what *poplăr* meant. Then the word had a sort of light-weight feeling. Knew something of the character of poplar wood; a light yellow—not visualized—the grain very slight. *S. I:* I knew that it could be easily cut, even against the grain. Some idea of the shape of a poplar: fairly heavy trunk, bushy at top like an oak, few branches. I was not thinking of the Lombardy poplar, but of the tree from which lumber comes. Although I did not see the wood, there was presented a board with a dark streak running through it.

The tendency to lengthen the word and the 'light-weight feeling' may have been kinaesthetic. It is more difficult to assign the presentations of the tree and the wood to such a source.

A striking case of pure description of an apparently imageless content is given in the following:

IV.43 *chauffeur,* 883σ. Had a presentation of a man sitting in an auto, one hand on the lever, one on the horn and wheel; goggles, cap, dust coat. Idea that his business was to run it. *S. I.:* Not seen. There was no auto. It was there but not in the field of attention. No reference to color, except that the dust coat was tan brown and so was the cap.

This report has the characteristics of a direct pictorial description, even to the discrimination of the objects in the field of attention from the auto, which was not. The long training of this observer hardly permits us to question the integrity of his report. If we decline to accept such an imageless report at its face value, the alternative is to suppose that the main period contained only a general sense of familiarity, together with certain tendencies of verbal expression which led to the report of the after-period. But to impose such an interpretation is to deny the validity of introspection altogether. There is no indication of *Kundgabe* here. It is a pure description of content, based apparently upon direct memory. If the

introspections were continuous with the experience, it might be difficult for the observer to separate the one from the other. But the reaction effectively separates these. Unless we challenge the integrity of the report we can hardly deny that contents representing the details given were actually present.

The next experiment is more complex in its associative tendencies, yet they are all closely connected with the nucleus. We find here again the selective action of attention as applied to imageless presentations:

IV.48 *coal,* 845 σ. Involuntary association between sound of word and color of apparatus in front. Did not recognize at time why it was made. Then *coal,* presented in small lumps as used in a grate: its blackness, irregularity, not haphazard because one of the grained sides—smooth—was presented. The next association was to a particular stove, and from that to a feeling that it gave out warmth, though it was a cook stove. I did not think of it as used for cooking, because I was trying to explain the use of coal, and it was not satisfactorily explained by cook stove. Tendency to exemplify heat, as felt, not for cooking, etc. This tendency was at the beginning. *S. I.:* No image of coal, but it was presented; the cook stove only in part. The coal was one piece which I know now was in a large basket. Knew then it was a part of something. Very intimate feeling with the word which may be explained from experience I have had in carrying coal, etc. Just the fire box of the stove. It was my aunt's stove in the kitchen.

This report is largely informatory. The four following factors may be analysed as present during the main period: (1) A tendency to connect the word with the black front of the exposition apparatus. The meaning of this was not given at the time, yet it appears to have been conditioned by the nucleus, since the dull black front of the card-changer resembled coal. The reason for such an association with 'the sound of the word' is not apparent, but an unconscious tendency to relate the two black objects would explain it. (2) The presentation of *coal,* described first as lumps, and then as one lump in a basket, indicates attention to the one lump, with its 'blackness,' 'irregularity' and one smooth, grained side, as emerging from its vaguely defined surround-

ings. (3) The association with stove, one part of which is attended to; and (4) the relation of this to *heat*, the dominating meaning of the whole experience.

The two following cases reveal this observer's ability to recollect a concrete situation without imagery:

IV.98 *clover*, 664σ. It seemed entirely new and of a different series. Placed me back seven or eight years, playing in a field; size, shape, slope, creek, row of trees and great enjoyment. Memory of the smell; bigness of things then—all this together. After reaction wished I could enjoy something as I did then. Then thought I would not exchange present enjoyments for those. *S. I.:* It was a clover field. No visual image or tactual experiences; some olfactory. It was as if I were on an eminence looking at myself there, as I was then.

This is clearly a case of association, for the meaning of the word does not appear to have been considered. A rather complex recollection is presented, as it were, in a nut-shell. How much of the detail was complete in the presentation, and how much was merely intentional, we cannot say, but the circumstantial nature of the report suggests precise facts rather than an amorphous content to be developed in the after-period. The second case involves a similar recollection:

IV.124 *willow*, 838σ. Seemed a new word. Tendency to look like a slender willow tree. Referred to a particular tree which I have, perhaps, not seen for twelve years, but around which I used to play. In the presentation:—the slightness of the branches, their 'give,' that the same branch would hold its size for some distance. *S. I.:* Not seen. Immediately after reaction there was presented the whole map of the country for two miles around that place: the lie of the land, things in separate fields. Feeling that I knew what was everywhere. The pond by the willow was presented in the main-period, a noticeable object. Remembered a lot of sandy mud near the tree, knew how one would sink in it as one walked to the tree.

The fact that the secondary report develops details and ascribes them respectively to the after-period and to the main-period, is an evidence of the integrity of this observer's introspection. Here again association is more prominent than relational meaning, yet the content of the main-period bears

more directly upon the word than it did in the preceding experiment.

In concluding these comments upon *D*'s introspections, note may be made of the brevity of his reactions in Series IV. No other of our observers approached his rapidity of mental action in any series. In this connection some comparative results are of interest with three other series of experiments in which the instruction was to secure visual, auditory and kinaesthetic images, respectively:

	Meanings (Series IV)	Visual Images	Auditory Images	Kinaesthetic Images
No. of Exps.	106	9	18	24
average	856.2σ	23,561.6σ	16,021.1σ	2,026.4σ
median	791.5σ	19,443σ	14,164.5σ	2,039σ
range	441σ-1,989σ	13,547σ-38,903σ	7,775σ-27,703σ	1,434σ-2,932σ

In the last three series the experiments considered were those only in which the task was successfully performed. The difficulty of securing visual and auditory images is thus indicated. We see that only with the kinaesthetic imagery did the reactions approach in brevity those for 'meaning,' and these are more than twice as long as those of the meaning series. The results are only suggestive as to the relative difficulty of the various tasks. It should be noted, however, that fifty experiments were conducted with kinaesthesis as the obect, although but twenty-four were productive of kinaesthetic imagery. The average for the whole number was 2,811.1σ. In view of the number of experiments it can hardly be said that the longer time was altogether due to lack of practice with this task.

V. *Conclusions*

The following tentative conclusions are suggested:

1. The meaning of a word is given in a mental act of relating.

2. The terms of the relation may be the word and a second content emerging from the dispositional matrix we have called the *nucleus;* or they may both be contents emerging from this matrix.

3. Since the nucleus is formed by the joint influence of the

instruction and the word, it determines the associative range which the word may have.

4. The contents which emerge are therefore associates of the word, or of some aspect of the instruction, or of the surrounding circumstances.

5. The mental act of relating is selective, and thus the relevancy or irrelevancy of the emerging contents is felt.

6. When the instruction to secure a meaning is dominant, only relevant contents are likely to emerge. Often this is not so, in which case the process is more that of associative recall and elaboration. The latter may suggest its own relevancies and subsidiary meanings without the essential meaning of the word being secured.

7. In addition to the registration of meaning by the emergence of a general or specific content related to the word, there is also a tendency of *expression, illustration* or *example.* This leads to the appearance of other associative contents, frequently imaginal and verbal which, by virtue of relations established with the nuclear meaning, give precision to the thought, usually in an illustrative manner. These are not essential to the meaning, and are often felt to be inadequate to it. Yet they are the more obvious, definite and concrete experiences. Hence, if adequate, they often seem to make out the complete content.

8. There is, nevertheless, a distinction to be drawn between the nuclear meaning and these expressive addenda. The meaning process is not identical with the process of expression, even though introspection does not always succeed in separating the two. When the expression follows quickly as an image whose relevancy is immediately and satisfactorily established, the vaguer consciousness attaching to the nucleus is unobserved. It may, indeed, be unobservable, *i. e.,* unconscious. Yet numerous cases in which the expressive content fails to appear, or in appearing fails to express adequately what the nucleus intends, give evidence of the influence which the nucleus exerts, and of its reality as the basis for establishing meaning.

9. Regarding the contents of these reports, we have found much evidence for the existence of a notional order of ele-

ments. These embody relations of previous formation, without reinstatement of the contents upon which the relations were originally formed. Notions of color, spelling, objects 'in the round,' to borrow a sculptor's term, *i. e.*, with their three-dimensional relations simultaneously present, etc., are revealed as factors of thought. By reason of their scope and synthetic nature these notions effect a tremendous economy over the circumstantial, point for point correspondence of the image.

10. The function of the notion appears in the range of facts, the complexity of experience, the point of view which may thus be associated, revived and related as a simple concrete entity. Notions are associated and revived as images are associated and revived. They do not constitute a meaning for the word any more than an image does. They may prove irrelevant as the images often are. We have found many which seemed to replace images in cases where an image would have served the purpose quite as well. Both are *vehicles* of thought, *i. e.*, vehicles to carry as wagons do, but not with their own motive power as automobiles. Both are contents to be related and known. Yet the notion has the advantage over the image inasmuch as it is essentially a meaning derived from past experience. Its scope may be wide or narrow; its reference may be concrete or abstract; its significance may be vague, undeveloped, general; or precise, formulated, specific. All this depends upon its genesis. Whereas images always remain the circumscribed sensory residua of particular experiences, and are essentially meaningless; notions are cumulative results, constantly growing in definiteness and meaningfulness as they are modified by the new relations into which they enter with other contents. To use an image as a term in a relation, is to make it part of a meaning process. The notion is used in the same manner. But the notion is also itself a meaning: the abstraction of previously formed relations; the image is not.

11. As to the vaguer essence of nuclear meaning to which we have referred, it is more difficult to describe the various analytic details of such experiences. There is often evidence of *Bewusstseinslagen:* attitudinal states of 'familiarity' and

'recognition;' attendant sensory and imaginal data, kinaesthesis and color phenomena. There is evidence of Ach's *Bewusstheiten,* generalized indications of 'knowledge about.' There are also the various stages of awareness and potential knowledge which Westphal[14] has described. There are, finally, the *intentions* and *Wasbestimmtheiten* of Bühler,[15] the latter being practically what we have termed *notions.* A rough analysis of the nucleus, so far as it may be consciously presented, would doubtless include elements of sensation, image, notion and affection. The *meaning* which may be attached is occasioned by the relation or relations established and tending towards establishment among these elements. But the essential significance of the nucleus rests in its dispositional matrix. It is this that controls the conscious appearances, and such associative and relational activities as are set up.

12. To conclude, there are three distinct phases to the meaning: its matrix, its conscious appearance and its expression. The matrix, or nucleus is dispositional, and is conditioned by the instruction and the stimulus word. The conscious appearances are of varying qualities, related and unrelated, few or many. They are the conscious representatives of the meaning. The expression is a reaction. It is more precise, more restricted, and bears more directly upon the extended report or behavior which is to follow. It seems probable that mental work goes on within the nucleus prior to conscious presentations. It also goes on among the conscious contents and after the expressed images, words, or behavior, result. The nucleus is the foundation of an *idea.* The contents which it determines register the idea. The presentation of these and their formulation is not always distinguishable from the expressive reaction. Hence our ideas as experienced have usually at once something of a general range of meaning, and something of the more precise and illustrative detail which controls our mode of expression, behavior and report.

Further investigation is needed to study the genesis of

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[14] *l. c.* p. 229.

[15] K. Bühler, Ueber Gedanken. *Archiv für die gesamte Psychologie,* IX., 1907, pp. 349f.

meaning under simpler and more definite conditions of control. The word, as here presented, proved to be too complex and variable a stimulus. Many nuances of the experience were difficult to analyse. Many were doubtless lost altogether. The complexity of the mental acts involved was also confusing. A similar investigation under conditions of brief exposure with words or pictured objects would perhaps be conducive to a more precise analysis.

THE SENSE OF SOCIAL UNITY—A PROBLEM IN SOCIAL PSYCHOLOGY

By Robert H. Gault, Northwestern University

The members of the same family, club, profession, city, state or nation experience a sense of standing together in a peculiar relationship: they feel that they belong together: that they are in reality one body. So vivid is this experience on the part of the member of many a family, club, or profession that he cannot contemplate his actual or projected behavior at any critical juncture of his affairs without taking very earnestly into consideration the attitude or possible attitude of his confrères in reaction to his behavior. Inevitably, as a matter of course, he, at such junctures, has a very intense realization of the strength of the bonds by which he is bound to others. At times when a wave of public feeling spreads over the land, too, this same experience comes to the foreground vividly. In the ordinary course of events it is in the margin; but I beleive it is a correct statement that, within no given period do we have a sense of aloofness from others in *all* our relations. There is, of course, a host of stimulus-response relations which are wholly outside the scope of our subject. The sun shines upon me through my window and I draw toward it with satisfaction; the horses' hoofs clatter loudly upon the brick pavement and I resent the disturbance; the clock strikes eleven and I determine to hasten my writing so that I may be able to complete my task tomorrow. Such psycho-physical processes as these crowd every minute of our lives. They are not social experiences. They have nothing to do in the matter of determining a sense of social unity though they may indirectly suggest it. Thus the clattering hoofs may at once bring into my mind's view my newly arrived neighbor to whom I once described this as a quiet street. I am immediately brought to realize my responsibility for his discomfiture. In other words I realize that he and I stand in a

peculiar relation to each other; between us is a social unity or solidarity, as it is sometimes called. Illustrations of a similar felt relationship throughout a large group could be multiplied.

The question I am approaching in this paper is: Whence comes this sense of social unity; what composes it; how is it developed?

In the first place it does not emanate from a social mind[1] in the sense of a mind distinct from that of each individual in the group but similar to it, which is alleged to do for an assumed social organism what my mind seems to do for me, viz., to effect a synthesis which I call myself. Neither, secondly, does it come from the operation of a social soul. Psychological method offers no clue to the existence of such a mind or soul. In the third place this unity is not summed up in the possession by the group of a common language, common customs, common laws, art, literature, mental outlook, systems of political and religious thought, etc., etc. These surely are signs that there is a social unity, and once they have been developed they facilitate the further evolution of the sense of unity. But without at least a rudimentary consciousness of solidarity to begin with, it is doubtful whether a common language, etc., could ever develop. Nor, fourthly, is the principle of social unity to be found in the co-ordination of individuals in the activities of the work-a-day world.[2] These co-ordinations, just as language, art, law, custom, etc., are objective products of a psychological unity that antedates them. Once we have found the psychological core of the phenomenon we are investigating, we can, I believe, account for the objective appearance. As we have already said of language, art, etc., so here we can assert a reciprocal influence between our co-ordinations in everyday affairs on the one hand and our sense of belonging together on the other; but the co-ordinations are not primary. We must make our appeal to psychological analysis.

What then does analysis of the experience we have under

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[1] E. J. Urwick, *A Philosophy of Social Progress,* London, 1912, pp. 109f.

[2] Charles A. Ellwood, *Introduction to Social Psychology,* New York, 1917, p. 82.

discussion reveal? Let each one take an illustration from his own experience as a member of a closely knit club, society, or family. I have again and again observed my fellow members in their reactions to a great variety of situations so that now even though far separated from them, when this feeling of belonging to them arises, I find that I have in my mind's eye an image of their behavior as it occurs in response to a situation with which I am confronted, or which I am creating, or I have an anticipatory image of their behavior as it will occur later in response to the situation that confronts me or to what I am at this moment thinking, saying or doing. Their imaged behavior I may approve or disapprove. It may be like or unlike my own in similar circumstances. I do not mean that this imagery need exist in great vividness nor detail; it may be wholly marginal and in course of time as the group interrelations become more and more highly mechanized, the imagery may almost wholly fade away. But all the while it seems to be an essential feature of my sense of social unity or solidarity with the group. A second element that analysis reveals is a purpose or ideal, or a set of purposes and ideals and felt needs that I cherish, and evidences of which I observe in the behavior of other members respectively of the group to which I belong. The third and final essential element is affective or emotional. At those moments when my sense of belonging to others is most in evidence, there is at least a feeling of satisfaction, and it may be one of enthusiasm, that arises.

These elements are all illustrated in the experience of the Boy of '61 as he trained upon the village green. In his imagination he could see hundreds of thousands of other youths like himself at military drill from ocean to ocean (imagery of other's reactions); he realized that all were doing so in order that they might the more effectively obey the summons from Washington (Purpose), and with it all arose the great swell of enthusiasm within him (Emotional factor). He felt that he was a part of a great closely interlocked and co-ordinated group. Without that imagery of others and that realization of common purpose, there could have been no enthusiasm in his make-up; no patriotism, no loyalty. He would have been merely an isolated drudge. This enthusiasm in each

individual expresses itself in easily recognized, easily imitated, and hence easily imagined signs. Consequently the emotional factor is an important contributor to the development of a sense of social unity. Indeed without at least a moderate emotional intensity, it is doubtful whether such a sense could persist at all.

From the foregoing, I believe it is justifiable to draw an analogy. There is primitive man surrounded on all sides by evidences of natural forces, no one of which he understands in the sense in which we say it is comprehended by the more enlightened age in which we live. To that extent, in primitive man, the anthropomorphic disposition is unbridled. He stands in awe before the wind, the river, disease, life and death. He reads his own dispositions, greatly magnified, into these evidences of unknown forces. He observes that his neighbors are doing likewise—a sign that they too are moved as he is. By this token a psychological kinship or unity is established as a matter of course. Once a considerable group is included in this unity, it is but a short natural step to coordination in institutions for worship, for punishment, etc. This follows upon the realization of a common purpose or a common need. I do not mean to imply that this felt and observed reaction to the mysterious is the only psychological root of primitive social unity, but that among other roots this one looms large. It does so, no doubt, because of the prominence of the emotional element that is associated with it. This element makes the reaction especially observable and contagious. It contributes therefore to the individual's readiness in imagery of others' reactions, as described above, and therefore to the felt unity of the group. The mere perception of a similarity in bodily form and color, too, contributes to the same end.

Thus far the discussion suggests the "consciousness of kind" which Giddings calls the primary element that makes for social solidarity.[3] Indeed, in the main, that is just what we are discussing. The consciousness of kind, is a consciousness of likeness of form, appearance, purposes, needs, behavior, etc., and as such it is a primary factor in the sense of social unity. It antedates the organization of groups and therefore

[3] F. H. Giddings, *The Principles of Sociology*. New York, 1896.

it precedes conflict among groups which Gumplowicz[4] describes as the elementary social phenomenon. But ere long, as organizations become more and more complex, conflicts become more varied and intense and then the consciousness of difference as well as of kind becomes an element in our sense of social unity. This comes about as a consequence of numbers of experiences in which we have failed to satisfy our needs or attain our purposes; attainments which we have seen made successfully even by others whom we recognize as, at least in a psychological sense, very different from ourselves. From that moment we recognize the need for such persons and they, in our imagery, enter in with others, to the group of components of our sense of social unity. In our mind's eye we see them, either as contemporaries or as belonging to a future generation, reacting to situations that we confront, in ways that we could not; responding to our behavior; coming to our aid and so contributing to the realization of our purposes and the satisfaction of our needs. Thus the *imagery of others' reactions* includes the *consciousness of kind,* antedates cooperation, organization and conflict, and later enters into reciprocal relations with all of these.

As the race has progressively gained control over the forces of nature, and specialization has followed upon specialization, social unities have multiplied within the group and we have the class consciousness of the commercial, the professional, the laboring sections, etc., and happily it may be, a larger unity including and superimposed upon all of these. But in every instance it is a unity that is made possible only by reason of such mutual familiarity or understanding on the part of the members of the group or sub-group that each one can and even does represent to himself the reactions by which others of his class would respond or are responding to a given situation. Obedient to the general law of automatization, the whole process, in course of time, becomes so highly mechan-

[4] L. Gumplowicz, *The Outlines of Sociology* (English translation, by Frederick W. Moore, of *Grundriss der Sociologie.* Wien, 1895). *Bulletin of the American Academy of Political and Social Science,* No. 253. Philadelphia, 1899, pp. 145-150. See also L. Gumplowicz, Le rôle des luttes sociales dans l'évolution de l'humanité. *Annales de l'institut international de sociologie,* XI., 1907, pp. 131-143.

ized that even the vaguest imagery suffices for the sense of unity which we have under discussion.

In all the foregoing we have had in mind a unity among contemporaries. Obviously, if our analysis is correct thus far, our principle applies as well to social continuity or unity between or among successive ages. As we peruse the history, literature and other products of the civilization of a by-gone generation, we become acquainted with the makers of that civilization; we know their natural dispositions and other motives; their modes of thought, etc. Again and again we discover particulars in which they react as we do to similar situations. Our capacity for imaging them in one or another set of terms develops until we can put ourselves in their place and them in our place, so that our consciousness of unity with them is made up of precisely the same sort of components as those that enter into our sense of unity with our contemporaries. The continuity of laws and courts and other institutions or the modeling of institutions upon old copies is not in itself social continuity: it is an expression of a psychological continuity. That we do not feel a strong sense of unity with the Fijians of our time, nor a sense of continuity with the ancient Egyptians would seem to be a corollary to the foregoing.

All this may appeal to many as over intellectualistic. I do not believe it is. Surely we could hardly describe by that term the unity among members of a profession. There is an of-course-ness about it that is not intellectualistic at all excepting that it rests in a vague shadowy lot of images of others with whom we are co-ordinated and so, as far as this is concerned, it is in the same category as each one's consciousness of one's self.

This is a distinctly individualistic psychological basis. It does not at all imply the direct transference of a psychic influence from one mind to another which Ellwood seems to fear the psychologist is approaching in a discussion of our subject.[5] Neither does it imply the mind of a social organism, nor anything of the sort, which co-ordinates and combines the individuals. Each and every member of the closely knit society to which I belong would be completely isolated men-

[5] *Op. cit.*, p. 79.

tally; there would be no unity among us were it not for the fact that we are all responding to similar situations so that in course of time each is able to represent the others and each knows approximately what to expect of the others.

What a student of psychology, in the light of the foregoing will have to say concerning the biological or race factor and the institution, organization or co-operation factor, to which Ellwood refers,[6] as determinative of a social unity or solidarity, is now apparent. In as far as we are of the same race, we can imagine one another with the greater facility; in as far as we co-operate in the same organization or institution our opportunities for observing one and another in action and for comparing reactions are enhanced. By reason of these associations, therefore, our anticipatory and other imagery of interactions among our confrères become more rich and complete and the emotional or affective components of the sense of unity grow apace. In short, then, the factors named merely furnish an opportunity for the development of the elements that enter into the sense of social unity. These factors, with the exception of the biological or racial one are a consequence of a felt unity that antedates them, and with which, from the moment of their inception, they are in reciprocal relation.

As I have said above, this is a strictly individualistic position. The student of the philosophy of law may at first glance find nothing in it to support more than individual or private interests whereas he is seeking a basis for the public interest. Our answer to him on this point is that "public interest" is only a phrase. What he means by "public interest" is only a series, so to speak, of widely overlapping private interests, or interests, held largely in common, in the same outstanding objects such as the right to life, health, property, freedom of speech, or in occasional subjects of political discussion, etc., etc. Because each one of us judging by indubitable signs, realizes, or judges after immediate inference, that the people around him in the neighborhood or in the state at large cherish the same main interests that he cherishes we have a public interest—of the only sort that we need. The way is then open for the pursuit of these interests in co-operation.

[6] *Op. cit.,* Chapter V.

A REVISION OF THE ROSSOLIMO TESTS[1]

By Herman Campbell Stevens, University of Chicago

Interest in mental tests is actuated by their practical bearings upon sociology and psychopathic medicine. While many tests for isolated psychophysical reactions have been devised and applied with a view to standardization, there are very few systems which have taken as their goal a complete and exact analysis of the mental functions. The Binet-Simon Scale, its more exact variant, the Yerkes-Bridges Point Scale and the Woolley tests are the most extensive undertakings yet essayed in this direction. How far they fall short is indicated by Yerkes[2] in his proposals for a Universal Scale which shall investigate four fundamental processes: (a) Receptivity, including such functions as sensibility, perceptivity, discrimination and association. (b) Imagination, including memory, in its various aspects, and constructive imagination. (c) Affectivity, including simple feeling, emotion, volition and suggestibility. (d) Thought, including ideation, judgment and reasoning.

Although Americans have been active in that part of applied psychology which includes mental tests, the interesting work of Rossolimo[3] is not generally known to them, possibly

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[1] From the Psychopathic Laboratory, University of Chicago.

[2] R. M. Yerkes and others, *A Point Scale for Measuring Mental Ability,* 1915, p. 165.

[3] A thorough acquaintance with the method and materials can only be obtained from the original papers, of which there are four: G. Rossolimo, Die psychologischen Profile: Zur Methodik der quantitative Untersuchung der psychischen Vorgänge in normalen und pathologischen Fällen: Eine experimentell-psychologische Skizze, *Klinik für psychische und nervöse Krankheiten,* v-vi, 1911, pp. 249-295; Die Formel des Profils, *Ibid.,* pp. 295-326; Die psychologischen Profile: Typen von Profilen psychischminderwertiger Kinder, *Ibid.,* vii-viii, 1912,, pp. 22-27; Berichtigungen und Ergänzungen zur Methodik der Untersuchung der psychologischen Profile, *Ibid.,* 1913, pp. 185-190. A summary of these tests was published by Beryl Parker, The Psychograph of Rossolimo. *American Journal of Insanity,* lxxiii, 1916, pp. 273-293.

because the author writes from the standpoint of psychopathic medicine.

Inasmuch as the original series of tests requires three hours for completion, the present writer has felt that they are too bulky for practical clinical work. And while Rossolimo's tests were admirably conceived and clever, there are undoubtedly too many unnecessary repetitions and slight variations to warrant the extended time required for their completion. During the past two years, therefore, at the Psychopathic Laboratory of the University of Chicago and at the Psychopathic Hospital of Cook County, the writer has attempted a revision of the Rossolimo tests with a view to bringing the whole examination within the compass of one hour, without sacrificing the excellent features of the original series. In this work the author has been assisted by Miss Beryl Parker who has applied the test to about thirty normal students of the University of Chicago and to the psychopathic children seen at the clinic of the Psychopathic Laboratory. In the Psychopathic Hospital of Cook County, the author has applied the revised tests to dementia praecox patients. The results of this work will be published later; it may be said, however, in anticipation, that the psychograph of dementia praecox has certain constant and characteristic features which may serve to differentiate early cases of this disease from those confusing borderline cases of amentia.

Nine mental functions were selected by Rossolimo as the fundamental processes to be tested. These processes are: attention, suggestibility (called by Rossolimo, will or tonus), recognition and discrimination, memory, comprehension, construction, mechanical ingenuity, imagination and observation and reasoning. Doubtless much may be said in criticism of this classification. There is considerable overlapping of the various classes. However, owing to the intimate relation of the mental processes, it is difficult to see how this fault can be entirely avoided in any comprehensive system of mental tests. Rossolimo's scores are expressed on a basis of ten as the maximum. In my revision, I have taken one hundred as the maximum score for each part of the test. This number lends itself somewhat more readily to the calculation of per-

centages, and at the same time it gives the same *relative* result as that expressed on the basis of ten. The psychographs obtained by the revised method are, therefore, directly comparable with those of Rossolimo. The psychographs are constructed by plotting the score of each test at the appropriate point on the ordinate, the abscissa of which is the function tested. A condensed psychograph, such as Figure 1, is made by averaging the scores of the various tests for attention, memory, etc., and plotting the resultant figure. The extended psychograph, such as Figure 3, is much more expressive since the result of each separate test of the function is represented. As evidence of the degree of agreement between the original and the revised method, the two condensed psychographs of Figures 1 and 2 are shown. The results may also be expressed as a formula which may be made clear by an example:

P 6.8=(7 5.5 8.2) 36.9%

The letter P, as Rossolimo uses it, stands for profile; as I use it, it means psychograph. The latter term is to be preferred to the former for the reason that it already has a definite connotation which is exactly applicable to this test. The number 6.8 is the average score of all the tests. The number 7 is the average score for the tests of attention and suggestibility. The number 5.5 is the average score for the tests of recognition and memory. The number 8.2 is the average of the scores of the remaining five processes. The number 36.9% is the percentage of loss, after one and a quarter hours, of the material memorized in the memory tests.

I. Attention

Test I. Attention

1. *Materials:* A 10 x 22 cm. card, upon which 50 3mm. holes have been punched at irregular intervals over the entire surface, a padded board 15 x 30 cm., an awl, a piece of loosely-woven silk cloth also 15 x 30 cm., and paper.

2. *Procedure:* A sheet of paper is laid upon the padded board and over it is laid, first the piece of silk, in order that the subject may not see the paper through the holes in the card, and then the card. They are fastened securely and the subject having been given the awl is told to punch through every

hole, beginning with the one marked with a star. The star marks the hole at the upper left-hand corner.

3. *Scoring:* A perfect duplicate of the pattern formed by the holes scores 100. Two points are deducted for each error, whether omissions or repetitions. Repetitions of course are the more difficult to detect, but it is very unusual for a second punch to be identical in position with the first. In case this should happen, however, a hole that has been duplicated will be noticeably larger than the single punches.

Test 2. Attention to exceptions

1. *Materials:* A card, 10 x 22 cm., in which 75 3mm. holes have been punched as for Test 1., a padded board, 15 x 30 cm., a piece of soft silk, of the same size, an awl, and paper. The holes in the card are marked, 50 with crosses, and 25 with a single vertical line.

2. *Procedure:* A sheet of paper is placed on the board, covered as for Test 1., with the silk, the card fastened on top of both. The subject is given the awl and told to punch every hole marked with a cross.

3. *Scoring:* The perfect score is 100, two points are deducted for each error, whether in punching a hole marked with a line or in failing to punch a cross.

II. Suggestibility

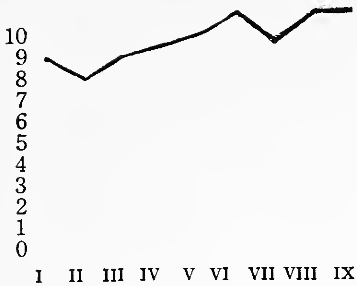
Test 3. Visual Suggestibility

1. *Materials:* A series of cards 5 x 13 cm., with a 1mm. line drawn lengthwise through the center of each. These lines all begin 1 cm. from the left-hand edge, and vary in length; one each of 5 cm., 6 cm., 7 cm., 8 cm., 9 cm., 10 cm.; and four of 11 cm.

2. *Procedure:* The first two cards are laid before the subject, the lines parallel. The examiner says, " Look closely. Is this line " (laying down the longer of the two) "longer than the one before?"

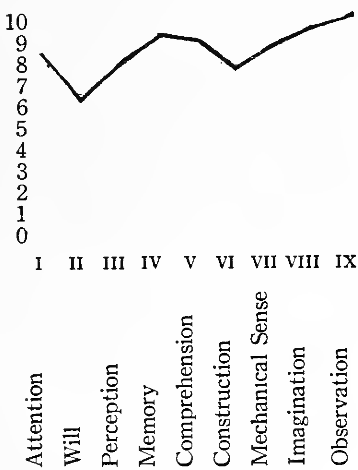
The same question is repeated as each successive card is compared with the one next shorter, until after the fifth card when the examiner changes to " And this?"

FIG. 1.



Norm for 6 persons, 8 to 32 years,
by original tests
P 9.2 = (8.5 + 9.2 + 9.7) + 9.1%

FIG. 2.



Norm for 6 persons, 8 to 34 years,
by modified tests
P 9.06 = (8.4 + 9 +9.3) +37 %

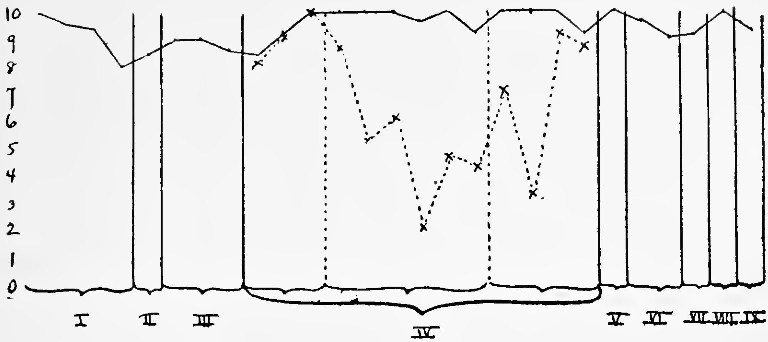
3. *Scoring:* Twenty-five points are deducted, from a perfect score of 100, for each time the suggestion of increasing length carries over beyond the seventh card.

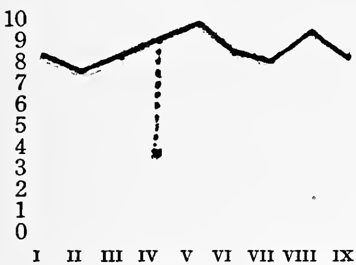
Test 4. Automatism in Action

1. *Procedure:* The examiner says, "With your right hand I want you to knock just as many times as I do. All ready. Begin." The examiner knocks ten times, but makes a motion as though to knock an eleventh time.

2. *Scoring:* The score of course is either 100 or 0, as the subject stops with the tenth knock or fails to do so.

FIG. 3.





Composite psychograph for 7 university students
P 9.3 = (8.9 + 9.3 + 9.5) + 34.7 %

Test 5. Kinaesthetic Suggestibility

1. *Materials:* Two wooden cylinders, 5 x 10 cm., of exactly the same weight and color. One of the cylinders has three large metal screws in each end.

2. *Procedure:* The two cylinders are given to the subject, who is asked to tell which is the heavier.

3. *Scoring:* 100 or 0, as the subject is or is not influenced by the screws to say that the cylinder containing them is the heavier.

Test 6. Pictorial Suggestibility

1. *Materials:* A picture in black and white, showing three mice on a block of wood, with other objects in the background. At one side is a very dark shadow which might by shape and size suggest a cat, but which very clearly is not a cat, or the shadow of a cat.

2. *Procedure:* The picture is exposed for 10 seconds. The subject is then asked to tell what he has seen, but is

interrupted with, "Yes, and on which side of the picture was the cat, left or right?"

3. *Scoring.* The statement that there was no cat of course is correct, score 100, otherwise 0.

Test 7. Suggestion by Sleight of Hand

1. *Procedure:* The examiner says, "I am going to repeat some names. You tell me how many I say." He repeats rapidly "John, Mary Susan, Kate," counting them off on *five* fingers, to suggest five as the number of names given.

2. *Scoring:* The perfect score is 100 if the subject maintains that only four names were given, otherwise 0.

III. Recognition and Discrimination

Test 8. Recognition of Figures

1. *Materials:* Five cards, 10 x 10 cm., are ruled off into nine equal squares. In each square is drawn a fantastic symbol of some sort. The character of the figures on the different cards varies, one having round line forms, another straight, another more or less geometric figures and combinations of figures, as a circle inside a square, etc., another scrolls, and another stars and dots in different combinations. All the figures on any one card, however, are very similar. On the back of each card is a duplicate of one of the nine figures on the face. (See Fig. 4.)

2. *Procedure:* The figure on the back of the card is exposed for three seconds, and the subject is then required to point it out from among the eight similar figures on the face of the card.

3. *Scoring:* Score is 100 or 0 as the subject is or is not able to recognize the figure which he has just seen.

Test 9. The Discrimination of Forms and Distances

1. *Materials:* Five cards, 7½ x 4 cm., upon which are drawn:

a. Two squares of equal size, one straight with the card, and one at an angle of 15 degrees.

b. Two parallel curved lines, one twice as heavy as the other, but equal in length.

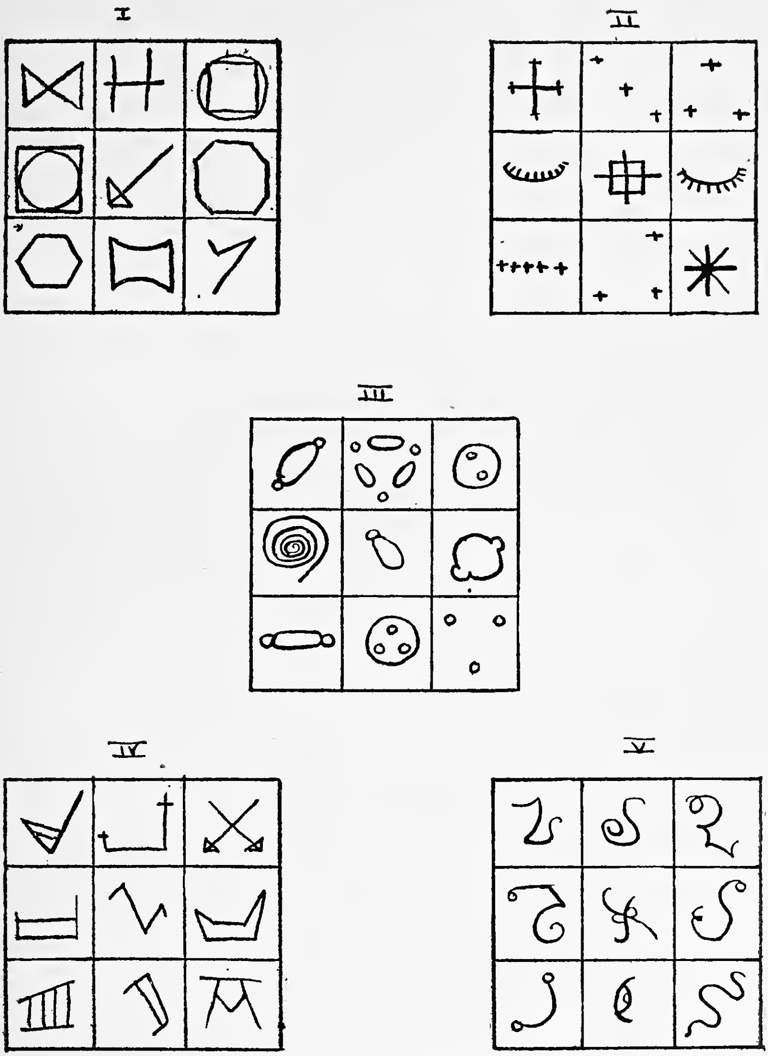


FIG. 4. ½ actual size

c. A row of 6 dots, all of the same size, but placed at irregular distances from one another.

d. A graduated spiral, and beside it a graduated waving line, the length of the two and the width, and the weight of the lines being exactly the same.

e. A series of vertical, 2 cm. lines, 12 in number, drawn lengthwise across the card at varying intervals.

2. *Procedure:* The cards are exposed singly, for three seconds, and the subject is then asked to state whether there is any difference or similarity in form, position, distance apart. It is not sufficient to say that there is a difference or a similarity—it must be specified in detail what the difference or similarity is, as for the first card, that the figures are alike and the same but one is tipped and the other straight.

3. *Scoring:* Perfect score is 100, twenty points to be deducted for each failure.

IV. Memory

Test 10. Memory for Linear Figures

1. *Materials:* A series of 12 cards, 8 x 8 cm., on which are drawn different irregular linear figures, as for example a gigantic square root sign, a rhombus lacking one of its sides, a zigzag line, etc., none of them, however, forming any completed, enclosed figure. (See Fig. 5.)

2. *Procedure:* Five of the cards are exposed for 3 seconds each and then shuffled into the remaining seven. The subject is then asked to select from the entire twelve the ones which he has first seen.

3. *Scoring:* The perfect score is 100, 20 points being deducted for each error.

Test 11. Memory for Colored Figures

1. *Materials:* A sheet of paper 15 x 20 cm., ruled off into 5cm. squares, each square containing a figure, irregular in shape and colored: red, olive green, orange, blue, lavender, green, purple, green, orange, red, rose, blue.

Five cards, 5 x 5 cm., bearing duplicates of five of the figures upon the sheet, one of the reds, one of the oranges, one of the blues, one of the greens and the purple. (See Fig. 6)

2. *Procedure:* The cards are exposed for 3 seconds each and the subject is then asked to indicate upon the large sheet which ones of the figures have been shown.

3. *Scoring:* Both color and form must be correct. The perfect score is 100, 20 points being deducted for each error.

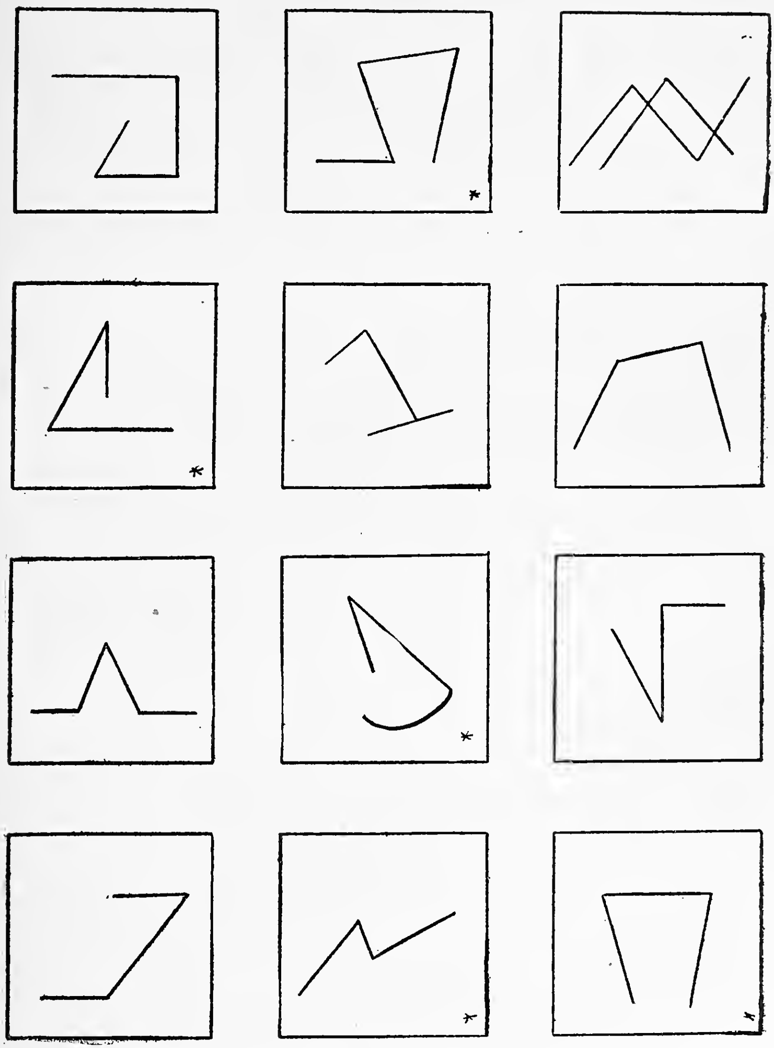


FIG. 5. [½ actual size. * indicates series of five.]

Test 12. Memory tor Landscape

1. *Materials:* Twelve postcard reproductions of landscape pictures, very similar in values and in placing of light and dark masses. Five of them shown to the subject, are: "Twilight," by Charles H. Davis; "Catskill Mountains," and

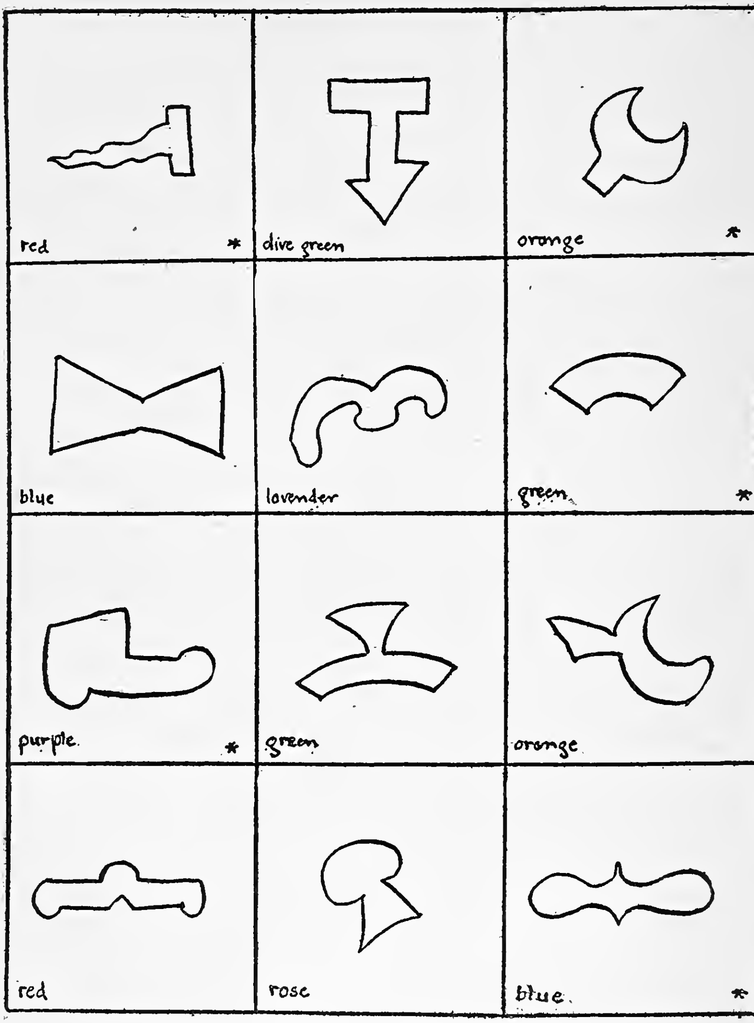


Fig. 6. [actual size. * indicates series of five.]

" The Home of the Heron," George Inness; " Pasture on the Road from Honfleur to Villerville," by Constant Tryon; and " Spring " by Rousseau. The other seven may be any similar pictures.

2. *Procedure:* The five marked cards mentioned above are shown to the subject for three seconds each, after which they

are shuffled with the other seven. The entire twelve are then exposed singly, the subject having been told to select the ones which he has seen before.

2. *Scoring:* The perfect score is 100, 20 points being deducted for each error.

Test 13. Memory of Letters Seen

1. *Materials:* Series of five cards, 5 x 5 cm., bearing the letters E, L, S, H, and V.

2. *Procedure:* Each card is exposed for three seconds, and the subject is then asked to repeat the letters which he has seen.

3. *Scoring:* Perfect score is 100—20 points off for each error.

Test 14. Memory for Letters Heard

1. *Procedure:* The examiner slowly pronounces the letters,—M, P, G, O, K. The subject is then asked to repeat what he has heard.

2. *Scoring:* Perfect score is 100—20 points off for each error.

Test 15. Memory for Words Seen

1. *Materials:* A series of cards, 5 x 5 cm., upon which entirely unrelated words are printed,—DOLL, VERY, SUN, MAY, RUN.

2. *Procedure:* The cards are exposed for three seconds each, after which the subject is asked to repeat the words he has read.

3. *Scoring:* The perfect score is 100—20 points are deducted for each error.

Test 16. Memory for Words Heard

1. *Procedure:* The examiner pronounces, slowly and distinctly, the words: WILL, CAT, CUP, TOWN, HEAR. The subject is then asked to repeat them.

2. *Scoring:* The score is as before, 100 for perfect, and 20 points deducted for each mistake.

Test 17. Memory for Sentences Seen

1. *Materials:* Five cards, 5 x 5 cm., bearing the sentences: HEAR THE BELLS. SEE THE SUN. BIRDS FLY. RUN AWAY. HIS TOP SPINS.

2. *Procedure:* The cards are exposed for three seconds each and the subject is then asked to repeat the sentences.

3. *Scoring:* 100 points for a perfect score; and 20 off for each error.

Test 18. Memory for Sentences Heard

1. *Procedure:* The examiner pronounces, slowly and distinctly, the sentences: COME TO ME. THE DOOR IS OPEN. HE WAITS. WATCH HIM. STOP THE TRAIN.

The subject is then asked to repeat the sentences.

2. *Scoring:* Perfect score of 100 points with 20 points deducted for each mistake.

Test 19. Memory for Numbers Seen

1. *Materials:* Cards, five in number, 5 x 5 cm., bearing the numbers:

33 — 9 — 2 — 7 — 15.

2. *Procedure:* The cards are exposed for three seconds each, and the subject is then asked to tell the numbers which they bore.

3. *Scoring:* Perfect score, 100; 20 deducted for each error.

Test 20. Memory for Numbers Heard

1. *Procedure:* The examiner pronounces slowly and distinctly, the numbers: 10 — 46 — 8 — 5 — 20. The subject is then asked to give them.

2. *Scoring:* Perfect score 100; 20 off for each mistake.

Test 21. Memory for Objects Represented

1. *Materials:* Five cards, 9 x 4 cm., bearing a certain number of identical representations of different objects: 3 rabbits, 5 candlesticks, 2 trees, 4 profile outlines of faces, 1 house.

2. *Procedure:* The cards are exposed, singly, for three seconds, after the subject has been told to remember both the name of the objects represented and the number of representations of each. He is then asked to tell what he has seen; "Three rabbits," etc. After he has told all that he can remember the examiner, if some are omitted, asks, "How many trees were there?"

3. *Scoring:* No credit is given if the subject remembers the object represented but fails to remember the number. The perfect score is 100, each card counting twenty, if described correctly, or ten if the subject can answer correctly the question "How many —— were there?"

Test 22. Memory for Actual Objects Seen

1. *Materials:* Five groups of objects; 3 bottles, 1 sugar-lump, 6 buttons, 4 dolls, 2 boxes.

2. *Procedure:* Each group is exposed for three seconds, after which the subject is asked to name the objects seen and give the number of each. If any group is omitted the examiner may ask as before, "How many —— were there?"

3. *Scoring:* The perfect score is 100, 20 points being deducted for each group not remembered, or 10 if the subject can recall the number of objects when questioned. If the names of the objects are remembered and not the number no credit is given.

This concludes the tests for immediate memory. At the end of one hour Tests 10-22 are repeated to determine the percentage of loss. These tests are given as follows:

Tests 10-12. Retention After One Hour

1. *Materials:* The materials are those of Tests 10, 11 and 12; linear figures, colored figures and landscape cards.

2. *Procedure: a.* The subject is shown the entire twelve linear figures and asked to select those five which he was first shown. See Test 10.

b. He is then given the large sheet showing the entire twelve colored figures, and as before asked to indicate the five which he was first shown. See Test 11.

c. He is then shown the twelve landscape cards and asked to indicate the five which he was first shown. See Test 12.

3. *Scoring:* Each of the three parts is considered as a separate test and scored as when used for the first time, 20 points being given for each correct selection; 100 is the perfect score for each of the five, i.e., 100 for *a*, 100 for *b*, and 100 for *c*.

Tests 13-22. Retention After One Hour

1. *Procedure:* The examiner asks the subject to:

"Tell me all the single letters you saw on cards."

"Tell me all the letters you heard me speak."

"Tell me all the separate words you saw on cards."

"Tell me all the separate words you heard me speak."

"Tell me all the short sentences that you saw on cards."

"Tell me all the short sentences that you heard me speak."

"Tell me all the numbers that you saw on cards."

"Tell me all the numbers that you heard me speak."

"Tell me what the pictures were that you saw on the cards and how many there were of each thing."

"Tell me what the real objects were which you saw and how many there were of each."

2. *Scoring:* At whatever time the subject recalls a letter, word, sentence or number in answer to a previous question it is recorded. The order makes no difference; and this should be explained to him at first.

As in scoring the tests when first given, each item correctly enumerated gives credit of 20 points; 10 points are given here also if he is able to give the correct number of objects in the last two tests, when asked, for instance, "How many houses were there?" In giving the examination to children who cannot yet read or to older persons who are illiterate, the tests requiring reading ability should be omitted, in order that the lack of education may not be counted as defective mental capacity. The spaces for these tests may be left blank on the record sheet. The auditory and visual stimuli which are independent of training are presented. Then the memory scores, immediate and retentive, and the percentage of loss are computed as if the tests were complete.

V. Comprehension

Test 23. Picture Interpretation

1. *Materials:* Three color prints:

a. Murillo's "Pastry Eater": two beggar boys, one of them holding up a piece of pastry to eat it, a dog, a basket of fruit and a loaf of bread wrapped in a cloth.

b. "Kinderstube" by Fritz von Uhde: three children at play with dolls, and toys scattered over the room and a woman seated at one side sewing.

c. A girl setting the table, and a cat rubbing against her, begging.

2. *Procedure:* The pictures are shown to the subject, one at a time; and he is asked to tell what each is about. He either

a. Simply enumerates the objects in the pictures; or

b. Describes the objects or action; or

c. Interprets the picture.

In reply to the questions,

"What is this picture about?"

"What would be a good name for this picture?"

"Will you tell me a little story about this picture."

3. *Scoring:* *a, b,* and *c* are scored as separate questions, the perfect score for each being 100 points; 10 points are given for the simple enumeration, 30 for description and 50 for interpretation. This makes a score of only ninety points for each. The additional ten points necessary for a perfect score are given only for some unusual degree of elaboration or comprehension.

VI. Construction

Test 24. Figure and Object Picture Puzzles

1. *Materials:* Two cards 12 x 8 cm., one bearing a very much twisted and curved angleworm form; and the other a row of four sail-boats with flags flying from their masts. These are cut in sections,—the angleworm only in one direction, across the card, and the boats in both directions.

2. *Procedure:* The pieces are shuffled and given to the subject for arrangement. For convenience and standardiza-

tion of results a time limit of two minutes is placed on the work with each puzzle.

3. *Scoring:* A successful arrangement of the puzzle within the two minutes gives a score of 50 points for each puzzle, the perfect score for the test being 100.

Test 25. Figure Construction from Elements

1. *Materials:* A card 6 x 4 cm., bearing an outline drawing of an angular figure (see A. Fig. 7) and several 1 cm. squares, and equilateral triangles, whose sides are also 1 cm.

2. *Procedure:* The subject is given the small squares and triangles; and he is told to construct a figure identical to that upon the card. He is not allowed to fit the pieces onto the card but must only look at it. The same time limit is observed.

3. *Scoring:* The perfect score is 100. If the figure is constructed properly, save for the three triangles at the base, 50 points are allowed.

VII. Mechanical Ingenuity

Test 26. Mechanical Puzzles

1. *Materials:* *a.* Two metal rods as shown in 1, Fig. 7. The smaller one may be slipped on and off the larger, bent rod.

b. Two semicircular pieces of metal which may be fastened by a third straight piece in such a way as to form a complete circle. (2, Fig. 7.)

c. A metal spiral with a metal hoop which may be worked on and off. (3, Fig. 7.)

d. Jointed rods as 4, Fig. 7.

e. A punch, the wheel of which may only be moved by pressing back the button a, (See 5, Fig. 7).

2. *Procedure:* The puzzles are presented to the subject one at a time in the form in which they are shown in Fig. 7. He is requested to:

a. Remove the small rod and replace it.

b. Put the three parts together to form a circle that will hold together.

c. Remove the elliptical piece of metal from the spiral and

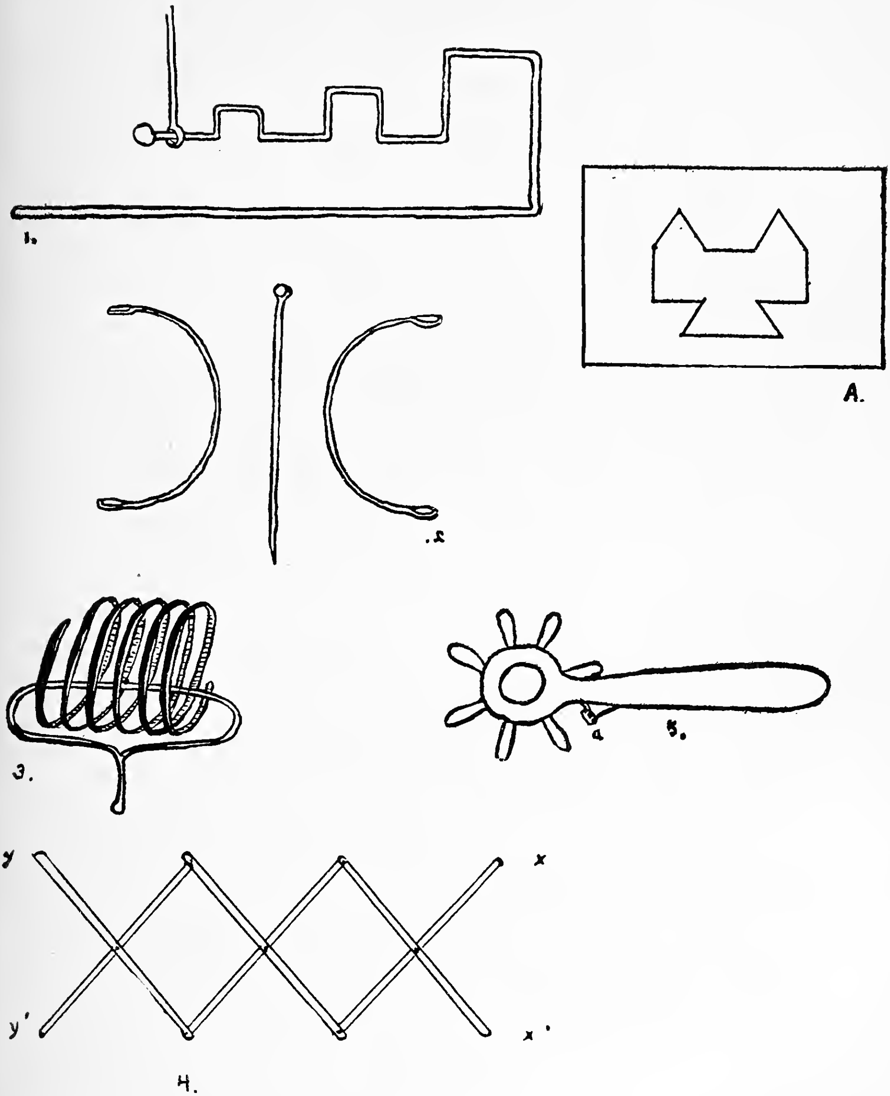


Fig. 7

replace it so that it encompasses each spiral as in the beginning.

d. To look at, but not touch, the jointed rods and tell what will happen to the points x and x^1 when the points y and y^1 are pressed together.

e. Turn the wheel of the punch.

3. *Scoring:* The perfect score is 100, 20 points being deducted for each failure.

VIII. Imagination

Test 27. Completion of Incomplete Pictures, Words and Sentences

1. *Materials:* Five cards, 18 x 12 cm., bearing:

a. A profile outline of a head, the line which should form the under lip, the bridge of the nose, the back of the head, the top of the ear and the front of the neck being omitted.

b. One foot and leg in a position of very active motion, a line to indicate the front of a wagon and one wheel of the wagon, at the proper distance behind the foot to show that it, completed, would be the picture of a boy running, and pulling the wagon after him.

c. The word, "GO–E—M–NT."

d. A Maltese cross, indicated but not completely drawn.

e. The sentence, "IF— DOES NOT – WE – – TO – COUNTRY."

2. *Procedure:* As the cards are presented to the subject, questions are asked as follows:

a, b, and *d*. "What would this be if the picture were completed?" The answers being "Face" or "Head;" "Boy pulling a wagon;" "Cross."

c. "What word would this be if a letter were put into each of those spaces?" "Government."

e. What sentence could you make by putting a word in each of these spaces? There are here of course several possible answers.

3. *Scoring:* The perfect score is 100, 20 points being deducted for each failure. In giving the test to small children and illiterates, *c* and *e* must be omitted.

IX. Observation and Reason

Test 28. Detection of Missing Number

1. *Materials:* Card bearing four rows of six digits, the only one missing being seven.

2. *Procedure:* The card is presented to the subject and he is asked what number under 10 is missing.

3. *Scoring:* Score is 100 or 0 as he detects or fails to detect that the seven is lacking.

Test 29. Detection of Form

1. *Materials:* A card 15 x 10 cm., upon which are a great number of different colored dots, the only regularity in their arrangement being that the green dots form a circle in the center of the card.

2. *Procedure:* The examiner asks whether the subject notices anything definite about the arrangement of the spots. If the subject does not, the examiner says, "Look closely at the dots. Which color is arranged so that not a single dot of that color is out of place? What is the form in which they are arranged?"

3. *Scoring:* The score is 100 or 0 as the subject does or does not discover the arrangement of the green dots.

Test 30. Reasoning from Observation of Detail

1. *Materials:* A picture representing a child carrying a basket. Just behind her appears a stream with high banks. She has just crossed it, for her foot-prints appear on both sides; but her dress is not wet.

2. *Procedure:* The picture is presented to the subject who is asked: "Do you think that the water is shallow or deep? Why?" He is supposed to observe the foot-prints and conclude that the water is shallow as the girl has walked across apparently without getting wet.

3. *Scoring:* Score of 100 or 0.

Test 31. Detection of Pictorial Absurdity

1. *Materials:* Two pictures showing:

a. Bald-headed man with a brush in each hand, standing

before a mirror brushing at his head. A stand in front of him contains more brushes and combs.

b. A woman seated holding a book as though reading. Her eyes are bandaged, however, and she wears spectacles over the bandage.

2. *Procedure:* The pictures are shown one at a time and the subject is asked what is there peculiar about them, or whether they are reasonable pictures.

3. *Scoring:* 50 points are given for each picture, the perfect score being 100.

This completes the description of the tests. A sheet for scoring is here appended.

Psychograph by Rossolimo Tests

Name	Date of Examination
Date of Birth	Examiner
School Grade	Diagnosis
Race	
Age	

I. Attention

Test 1. To punch holes without omission or repetition

Test 2. Attention to Exceptions. Marked holes only

II. Suggestibility

Test 3. Visual Suggestibility. Length of Line

Test 4. Automatism in Action. Knocking on Table

Test 5. Kinaesthetic Suggestibility. Cylinders

Test 6. Pictorial Suggestibility. Shadow—Cat

Test 7. Suggestibility by Sleight of Hand. Four Names, Five Fingers

III. Recognition and Discrimination

Test 8. Recognition of Figures

Test 9. Forms and Distances

a. Squares

b. Curved Lines

c. Row of Dots

d. Spiral and Waved line

e. Vertical Lines

IV. Memory	Immediate	After 1 Hour
Test 10. Memory for Linear Figures		
Test 11. Memory for Colored Figures		

Test 12. Memory for Landscapes
Test 13. Memory for Letters Seen
Test 14. Memory for Letters Heard
Test 15. Memory for Words Seen
Test 16. Memory for Words Heard
Test 17. Memory for Sentences Seen
Test 18. Memory for Sentences Heard
Test 19. Memory for Numbers Seen
Test 20. Memory for Numbers Heard
Test 21. Memory for Objects Pictured
Test 22. Memory for Actual Objects Seen

V. Comprehension

Test 23. Picture Interpretation
a.
b.
c.

VI. Construction

Test 24. Figure and Object Picture Puzzles
Test 25. Figure Construction from Elements

VII. Mechanical Ingenuity

Test 26. Mechanical Puzzles

VIII. Imagination

Test 27. Completion of Figures and Words and Sentences

IX. Observation and Reasoning

Test 28. Detection of Missing Number
Test 29. Detection of Form in Colored Dots
Test 30. Reasoning from Observation of Detail
Test 31. Detection of Pictorial Absurdity
a.
b.

THE AFFECTIVE TONE OF COLOR-COMBINATIONS[1]

By L. R. Geissler, Clark College

CONTENTS

I. Introduction

The present investigation was undertaken for the purpose of measuring, if possible, the influence of the affective tone of isolated colors upon the affective tone of combinations of these colors. That such an influence is operative in the preference

[1] This investigation was begun in March, 1915, and carried on chiefly in the Psychological Laboratory of the University of Georgia; but additional data have been accumulated at Clark College, during the early part of 1917.

of color-combinations has been admitted by all previous investigators of "color-harmony," as Wundt calls it; but none of these investigators has paid any further attention to it. The problem seems to us of far-reaching significance, especially when it is considered and stated in more general terms, viz., what is the influence of the affective tone of mental constituents, when experienced singly, upon the affective tone of their combinations, that is, upon their *Totalgefühl*?

The experimental work with color-combinations as well as the various theoretical considerations of the problem have been discussed at length by Wundt,[2] so that we need here only refer to it. Unfortunately, our predecessors have confined themselves to a much narrower aspect of the question in that they have attempted to determine whether combinations of complementary and "near-complementary" colors are more or less pleasant than non-complementary combinations. In other words, the problem has been attacked from the standpoint of colors or theoretical aesthetics rather than from that of the affections or psychology proper. To us, however, the behavior of the affections in complex mental structures is the significant object of study. The question foremost in our mind in undertaking this work could be stated thus: Do the affective tones of two (or more) constituents when experienced singly summate when experienced in combination, or do they average, or do they neutralize each other, or what happens to them?

To be sure, the answer will have to take into account the nature of the constituents to which the affections are attached, as well as the number of these constituents. On the basis of these two factors alone the following complications are theoretically possible: (a) two sensations of the same modality which vary in one attribute only, for example, colors which vary in quality or in spatial extent; (b) two sensations of the same modality which vary in more than one attribute, as colors of varying hues and tints; (c) two sensations of different modalities which vary in one attribute only, as tastes and odors, or touch and sight; (d) three sensations of the

[2] W. Wundt, *Grundzüge der physiologischen Psychologie,* 6te Aufl., III., 1911, pp. 129-134.

same modality which vary in one attribute but have a constant pattern of arrangement (of spatial, temporal, or other relations); (e) three sensations of the same modality which vary in one attribute and in the pattern of arrangement; (f) three sensations of the same modality which vary in more than one attribute but show a constant pattern; (g) three sensations of the same modality which vary in more than one attribute and in the pattern of arrangement; (h) three sensations of different modalities which vary in one attribute but retain constant patterns of arrangement; (i) three sensations of different modalities which vary in more than one attribute but show a constant pattern; (k) three sensations of different modalities which vary in more than one attribute and in the pattern of arrangement. Still further complications are possible by the introduction of imaginal processes into the combinations. Enough possible complications have here been indicated to point out the wide range and complexity of our problem.

The writer is convinced that this new approach to the study of the affections will prove more fruitful in the end than a physiological attack, especially if it is also to be extended to an ontogenetic line of work, to which he has already devoted some time. He furthermore hopes to derive from its continuation valuable hints and suggestions for an experimental attack upon the study of instincts. The present investigation is directed toward the first of the various theoretical possibilities. It confines itself to the pleasantness of two color sensations which vary in hue mainly but otherwise maintain constant spatial and temporal relations.

Since the emphasis of our work was to be put upon the affective character of a combination as compared with that of its constituents, our choice of colors was of secondary importance, and was determined mainly by the question of facility in handling them and of obtaining with as little loss of time as possible a fairly large number of observations from a considerable number of subjects. At any rate, our selection of colors and their number was not influenced by any psychological or physical theory of colors, although we aimed to use colors of nearly equal tint and chroma.

We desired to obtain as many data as possible from as large a number of subjects as possible in order by this means to evade one difficulty which, according to many writers upon the subject of affections, is inherent in the supposedly subjective character of these mental processes. It is frequently stated that while the same sensory stimulus will arouse in all normal individuals the same sensation, the affective tone of this sensation may vary not only from individual to individual, but also with the same individual from time to time. Furthermore, it is a well-known fact that affections by repetition will undergo adaptation, sometimes to the point of indifference, and sometimes even beyond this point into their opposites. This fact induced us to refrain from an intensive study of our problem with many colors and a few highly trained subjects and instead to use the extensive method of employing few colors but many less well-trained subjects. The former method as applied to the affections seems to us to have two other serious disadvantages. On the one hand, the results from a few subjects can not be generalized to the same extent as in the case of the sensations, on account of the lack or uncertainty of our knowledge regarding individual differences in this field. And on the other hand, affective abnormalities, analogous to color-blindness or tonal islands, etc., may occur and yet be much more difficult to discover than sensory defects. At any rate, the disadvantages of adaptation and individual differences were eliminated by the use of the extensive method which at the same time promised some interesting by-products about color-combination preferences as varying with sex, age, occupation, and the like.

II. Material and Method

The employment of less well-trained subjects does not involve any disadvantages in our particular study, since our method required of them only a relatively simple task. We obtained results from at first 90, and later from 32 additional subjects, an equal number of men and women, whose ages ranged between 17 and 40 years. As to occupation, they were either college or normal-school students or public school teachers, mostly single, although a few married people are

included.[3] Each subject made 210 comparisons of color-pairs and 21 comparisons of single colors, which gives a total of 25,620 comparisons of color-pairs and 2562 comparisons of single colors.

We used as our stimulus-material the following seven Milton-Bradley pigment colors and their twenty-one combinations: red, orange, yellow, green, bluegreen, blue, and purple. These colors were seen through two oblong openings in a large background made up of a medium-grey upright cardboard of 50 x 150 cm. Each opening was 5 cm. in height and 10 cm. in width, one placed above the other with a space of 5 cm. between them. For the comparison of single colors, which in the original 90 subjects preceded, the left half of each opening was closed by a small medium-grey piece of thin cardboard which was easily removed for the comparison of the color-pairs. Between exposures the openings were entirely closed by another movable, medium-grey cardboard, sliding in front of the large screen; this slide was manipulated by the Experimenter from behind the background in such a way that he could remain entirely invisible to the subjects during the experiment. He also manipulated the long narrow cardboard-strips which slid in horizontal grooves behind the openings and which carried the various pairs of color. The whole apparatus was set up in such fashion as to face the window without being exposed to direct sunlight. The subject, with his back to the window, faced the large background and recorded on specially prepared blanks his preference for the upper or lower stimulus by placing an L or U in the proper place. This device permitted us to experiment with a number

[3] The author gladly takes this opportunity to thank each and all of the participants in the experiments for their patient and conscientious co-operation, and he wishes in particular to acknowledge his indebtedness to President H. J. Pearce of Brenau College, Gainesville, Georgia; to Professor L. A. Averill of the Mass. State Normal School, Worcester, Mass.; to Mr. G. W. Walter, Graduate-Assistant in the Department of Psychology at the University of Georgia, who with much ingenuity and skill prepared the entire apparatus and stimulus-material and helped to conduct the experiment in several cases and to compute some of the data; and to his wife whose efficient and conscientious aid in computation contributed greatly to the accuracy of the results.

of subjects simultaneously, care being taken that all could see the stimuli without inconvenience and that no interference with each other occurred.

The method of paired comparison was used throughout, and the actual procedure for the whole experiment was as follows: The writer, who in all but four or five cases conducted the experiments himself, gave identical preliminary explanations and instructions to all of the subjects. The instructions called for an immediate judgment as to whether the upper or the lower of the two colors or color-pairs was more pleasing or better liked. Cases of doubt or indecision were to be indicated by a question-mark; but the subjects were always requested to use it but sparingly or only as a last resort. They were also warned to base their judgments entirely on what they saw and to exclude all other considerations or reflections. The negative form of the warning against associated ideas was purposely avoided in order to prevent suggestions. The subjects were told of the warning-signal, and a demonstration of the temporal sequences was given so that they might estimate how much time they had for judging and recording their choices. The experiment was not begun until everybody understood fully what to do. Each exposure was preceded by a warning-signal "Ready"; and one to two seconds afterwards the experimenter noiselessly moved the cover aside for a two-seconds' exposure. Then the stimulus was covered again; and while the subjects recorded their judgments the experimenter shifted the two strips for the next exposure. The apparatus worked almost noiselessly, and during a series of ten to twenty judgments nothing else was audible but the "ready" signal. At the end of that time a slightly longer pause had to occur during which the experimenter took out the old and inserted new slides. This change of strips never required more than 30 seconds. After half of the color-pairs had been exposed, a longer pause of five minutes was made, during which the subjects were warned not to talk to each other about the experiment. The whole experiment lasted from 50 to 75 minutes, according to the time consumed in the original explanations and demonstrations.

In the case of the 32 additional subjects the experiment began with the color-pairs, and the single colors were compared in the last series. This change was made in order to meet Miss Baker's[4] criticism of Cohn's[5] experiments, which had, however, not been accessible to us until after we had completed the experiments with our original 90 subjects. Miss Baker objects that "the relative agreeableness of the individual colours should not have been taken until after the judgments on the combinations had been given. There is in every judgment on a combination not only the affective tone of the combination but combined with this the affective tone of each of the individual colours. To call attention to the individual colours first gives the affective tone of these too prominent a place in the judgment." We shall later in this paper urge certain other considerations against this *a priori* criticism, but we knew that the only way to deal with it effectively is by an appeal to facts, and so we repeated our series with the additional set of 32 subjects. The outcome of both sets of experiments will be treated separately so far as that seems necessary.

III. Principles of Interpreting Data

Throughout the discussion of our data and results it must be kept in mind that the method of "paired comparison" as used in this investigation does not furnish judgments of indifference or of unpleasantness or dislike, but only judgments of relatively greater pleasantness. Hence we cannot say that a color or color-pair which was never chosen by a subject was indifferent or even unpleasant to him; we may only say that it was "least" pleasant to him. Now it is obvious that with either single or paired colors only one of them may have been never or always preferred, and we shall call such cases the *absolute* minima or maxima, or single extremes. With the 7 single colors the single extremes are 6 and 0, with the 21 color-pairs they are 20 and 0. On the other hand, if either or

[4] Emma S. Baker, Experiments on the Aesthetics of Light and Colour. First Article, On Combinations of Two Colours, *University of Toronto Studies, Psychological Series*, I, 1900, p. 41 (217).

[5] Jonas Cohn, *Philosophische Studien*, X, 1894, pp. 562-603.

both of the single extremes do *not* occur among the preferences of a given subject, several colors or pairs may be chosen by him with a frequency next in rank to the absolute maxima or minima, and we shall call such cases the *relative* maxima and minima, or divided extremes. With regard to the intermediate frequencies it is admissable to treat each as a different degree of pleasantness; but such a procedure would make it practically impossible to compare the intermediate degrees of the single colors with those of the paired stimuli. Hence we decided to use only three degrees of pleasantness, highest, medium, and lowest. The number of times which a single color has been preferred over other single colors may be called the "individual frequency" of that color.

There are two principles of deciding which frequencies should be included in each of the three degrees of pleasantness. According to one principle the least pleasant color is indicated by the absolute or relative minima; similarly the most pleasant color is represented by the absolute or relative maxima; and the medium pleasant colors are of intermediate frequency. We have called this determination the principle of "varying extremes", because for one person the most pleasant color may have an "individual frequency" of 6, for another, one of 5, for still another, one of 4; and likewise for one person the least pleasant color may have an individual frequency of 0, for another, one of 1, and for still others one of 2 or 3.

According to the second principle, that of "constant extremes," only such colors as occur with "individual frequencies" of either 6 or 5 are called most pleasant; likewise those of an "individual frequency" of either 0 or 1 become the least pleasant colors; and the "individual frequencies" 2, 3, and 4 represent then the medium-pleasant colors. When this principle is applied any given subject may or may not present cases of most or least pleasant colors, and the results must here be grouped or classified according to cases rather than according to individual subjects, as is possible with the principle of "varying extremes." As we could not find offhand any definite advantages in favor of either principle, we decided to compute our results, at least from the original 90

subjects, according to both principles and to determine whether they would present any characteristic differences. Since such differences could not be found, we calculated the rest of the results according to the principle of " constant extremes," because of its greater simplicity and directness.

Our problem may now be restated in the following concrete form: What degrees of pleasantness will result for color-pairs if they are composed of (a) two most pleasant colors, (b) a most and a medium pleasant color, (c) two medium pleasant colors, (d) a most and a least pleasant color, (e) a medium and a least pleasant color, and (f) two least pleasant colors ? The degree of pleasantness of the color-pair may be expressed most simply by the average frequency of all the cases falling into each of these six combinations.

In calculating our results according to the first principle, that of "varying extremes," we found that our 90 subjects could be divided into four groups or types with regard to their choices of single colors: Group A, containing 25 men and 24 women, showed both absolute maxima and minima or decided extremes; group B, including 6 men and 6 women, were decided as to their maxima but divided as to their minima; group C, with 8 men and 11 women, showed the reverse relation,—divided maxima but decided minima; and group D, including 6 men and 4 women, were the undecided type, showing divided maxima and divided minima. It will be necessary to refer to these groups, because only by thus dividing our subjects will it be possible to obtain the six combinations (a) to (f). For example, groups A and B cannot furnish examples of combination (a), while groups A and C cannot give combination (f). Since therefore the combinations (a) and (f) were not very frequent among our original 90 subjects, we decided to compare first of all the average frequencies of combinations (b) and (e), in each of which a medium pleasant color occurs, combined, however, in (b) with a most pleasant color but in (e) with a least pleasant color. Using the principle of "varying extremes" we found that 79 of the 90 subjects showed a greater average frequency for (b) than for (e), and we shall refer to these subjects as "typical" and to the remaining 11 as "reversals," because

with the former class the same relation was discovered to hold throughout all the other combinations, as a more detailed study of our data and of the figures in Table I will reveal.

IV. Law of the Summation of Affections

Throughout the whole Table the figures in parentheses indicate the number of cases included under a special rubric. The Table is subdivided vertically into the six possible affective combinations of color-pairs. The horizontal subdivisions give in the upper half the nature of the group of subjects or cases; and in the lower half the average of each group and its mean variation. The data as calculated by the method of "varying extremes" are enclosed in the two blocks surrounded by heavy type lines. Here the capital letters A, B, C, and D refer to the four groups of "typical" subjects, while the letters A-r, B-r, and C-r refer to the "reversals"; D-r does not occur. The averages in all rubrics are given on the basis of the number of cases only, although they have also been calculated on the basis of the number of subjects; the differences between these two treatments of data were however very slight, involving a change in the decimals only.

The data of Table I enclosed within the heavy lines may then be read and interpreted in the following way: Let us take for example column 2, rows 16-17; the subjects showing divided maxima or several equally "most pleasant" single colors give to the paired combinations of these colors an average frequency of 13.9, with a mean variation of 3.16. The maximal frequency which such pairs could have obtained is of course 20; but this figure could not even be logically expected, since in no case could one of the single members of a pair have an "individual frequency" of 6. The best possible "individual frequency" combination could be 5+5. To return then to our case of 13.9, we find it is never exceeded in the blocked off data by any other average except in the case of the same column, rows 18-19, with group D, whose average is 14.1. This is, however, a confirmation of our previous result, viz., if two individually most pleasant colors are combined, this pair is on the average more pleasant than any other color-pair whose members have individually lower

degrees of pleasantness. This statement remains true even if the results of the "reversals" (col. 2, rows 12-13) are included in the general age, which is shown in rows 24-25.

A further inspection of the "typical" cases reveals a still greater uniformity in the gradual decrease of the averages, as colors are paired whose individual affective values are lower and lower. Thus the averages of the most-medium pairs range in the four groups from 13.8 to 12.0, (col. 3, rows 13, 15, 17, and 19), while the medium-medium pairs range between 11.4 and 8.3; as such they do not vary from the most-least combinations, which range between 9.7 and 9.0. This lack of a difference here may logically be expected, since two medium degrees of intensity may be said to average the same as a maximal and a minimal intensity. The averages continue then to decrease by a rather large step for the medium-least combinations, which again are somewhat above the least-least pleasant combinations. The same results are reflected in the general averages which include the data from the "reversals." These figures force upon us the general conclusion that *the pleasantness of the color-pairs increases directly with the pleasantness of the colors taken individually.* This is then the answer to our problem as restated on page 158.

Before we accept this result as final, let us inspect our data as computed by the method of "constant extremes" and as presented in the rest of Table I. The grouping of the data here is entirely on the basis of the absolute "individual frequency" of each member of the pairs, and this "individual frequency" is indicated in each group by such symbols as 6+5, or 4+4, or 2+0, etc. Accordingly the data of this part of Table I must be read and interpreted in the following way: Take for example column 3, rows 6-7; here we have 55 cases or color-pairs in which the "individual frequencies" are 6 and 3 and the average of these pairs is 13.4 with an m.v. of 3.2. In the neighboring group, column 4, rows 6-7, the "individual frequencies" are 5 and 4, that is, their sum is the same as 6 and 3, and we may therefore average these two groups, as is done in column 12, rows 6-7. Likewise in the rest of this column all the other rows are averaged, whose individual frequencies total the same amount. For example,

TABLE I

	1	2	3	4	5	6	7	8	9	10	11	12		13	14
1		Most + Most	Most + Medium		Medium + Medium		Most + Least		Medium + Least		Least + Least	90 Subjects Av.		32 Ss Av.	122 Ss Av.
2 3	11	6+5, (34) 15.1±3.7			A, (430) 10.2±1.3	A—r, (60) 10.8±.9	A, (43) 9.5±3.5	A—r, (6) 9.5±4.5	A, (215) 6.5±3.2	A—r, (30) 11.1±3.5		11	1 Group (34) 15.10	(12) 17.75	(46) 15.8
4 5	10	5+5, (33) 14.7±3.3	6+4, (54) 14.1±3.5		B, (63) 11.4±4.1	B—r, (6) 6.8±2.5	B, (23) 9.7±4.2	B—r, (2) 16.5	B, (89) 8.1±3.6	B—r, (8) 11.7±3.2	B, (13) 6.8±4.6	10	2 Groups (87) 14.33	(40) 15.15	(127) 14.6
6 7	9		6+3, (55) 13.4±3.2	5+4, (78) 12.8±3.4	C, (78) 8.3±4.0	C—r, (18) 9.5±3.0	C, (34) 9.0±3.4	C—r, (10) 11.0±3.4	C, (56) 6.9±3.3	C—r, (14) 12.8±4.4		9	2 Groups (133) 13.05	(53) 12.90	(186) 13.0
8 9	8		6+2, (73) 12.1±3.5	5+3, (122) 11.9±3.5	4+4, (58) 11.4±3.4				D, (50) 6.6±3.1		D, (16) 6.0±3.0	8	3 Groups (253) 11.80	(76) 12.47	(329) 12.0
10 11	7		5+2, (117) 10.5±3.7		4+3, (116) 11.2±3.7		6+1, (43) 11.5±4.1		4 Groups (410)	3 Groups (52)	B+D, (29) Av. 6.5	7	3 Groups (276) 10.95	(98) 11.03	(374) 11.0
12 13	6	C—r, (4) 12.1±2.1	A, (215) 13.8±3.2	A—r, (30) 7.1±2.8	4+2, (121) 10.8±3.9	3+3, (55) 10.6±3.5	6+0, (34) 10.9±2.6	5+1, (104) 9.3±3.7	Av. 6.9		B—r, (1) 17.0	6	4 Groups (284) 11.36	(109) 10.10	(393) 11.0
14 15	5		B, (43) 13.2±4.0	B—r, (4) 6.2±3.9	3+2, (123) 8.8±3.9		5+0, (64) 8.7±3.8		4+1, (109) 8.7±3.6			5	3 Groups (296) 8.74	(104) 9.00	(400) 8.8
16 17	4	C, (23) 13.9±3.16	C, (124) 12.0±1.8	C—r, (34) 9.0±3.8	2+2, (46) 7.8±3.9				4+0, (72) 8.2±3.5	3+1, (91) 7.2±3.1		4	3 Groups (209) 7.67	(76) 6.75	(285) 7.4
18 19	3	D, (19) 14.1±4.2	D, (50) 13.2±3.3	D—r (0)	D, (19) 10.8±4.1				3+0, (77) 7.7±3.2	2+1, (86) 7.2±3.4		3	2 Groups (163) 7.49	(58) 5.89	(221) 7.0
20 21	2	2 Groups (42)	4 Groups (432)	3 Groups (68)	4 Groups (590)	3 Groups (84)	3 Groups (100)	3 Groups (18)	2+0, (67) 6.6±3.6		1+1, (26) 6.4±3.5	2	2 Groups (93) 6.56	(31) 5.67	(124) 6.3
22 23	1	Av. 14.0	Av. 13.2		Av. 10.0		Av. 9.5				1+0, (34) 6.1±3.5	1	1 Group (34) 6.10	(15) 4.47	(49) 5.6
24 25	Av.	All (46) 14.0	All 7 Groups (500) 12.5		All 7 Groups (674) 9.24		All 6 Groups (118) 9.74		All 7 Groups (462) 7.42		All (30) 6.9	26 Groups Total (1890)		(672)	(2562)
26 27 28	Av.	14.9 6+5&5+5 (67)	11.1 All 6 Groups (499)		10.2 All 6 Groups (519)		9.76 All 4 Groups (245)		7.67 All 6 Groups (502)		6.23 1+1&1+0 (60)	2562 Preferences of Single Colors 25620 Preferences of Color-Pairs			

in row 15 of column 12 we find averaged the three groups totalling 5, namely 3+2, 5+0, and 4+1. Comparing now the averages of the groups as they are arranged in column 12, we find here a surprisingly steady decrease (with one slight exception in row 13) from 15.1 to 6.1 which is paralleled by another arrangement of the same data as indicated in row 26, where the groups are averaged according to the six combinations of degrees of pleasantness. Here again we have a most uniform decrease from 14.9 to 6.23, which is carried out even within the individual groups themselves. The method of "constant extremes" furnishes therefore a most extraordinary confirmation of the results as found by the other method.

The data which we have discussed so far were obtained entirely from our first set of subjects. In column 13 we present the data from our 32 additional subjects who, it will be remembered, began the experiment with a comparison of the color-pairs and who judged the single colors last. These data were treated only by the method of "constant extremes" and the averages analogous to those in column 12 are listed in column 13. Instead of invalidating our previous results they offer a still better sequence of gradual decreases, which goes to show that the degree of pleasantness of color-pairs is not influenced by the previous exposure of the isolated colors. We may therefore combine the data of the first and second set of subjects, as is done in column 14, and on the basis of this complete evidence restate our previous conclusion in the form of a general law of the summation of pleasant affections of single colors in color-pairs which may be formulated as follows: *The greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination.*

Neither the writer, who belongs among the 11 "reversals," nor any of the 90 subjects who offered any comment on the experiment after participation, had anticipated such a result. We believe that we have established this law beyond a doubt for the material of color-pairs; and the question of the extent or range of the applicability of the law is merely a matter of accumulating new data with the unpleasant affection, with

different kinds of sensory material, and with younger subjects. We have already made a beginning in this latter direction with a child first at the age of three and a half years, and then at intervals of nine months; and we are planning to continue this work with other young children. The data so far accumulated show, with even greater consistency than with adults, the working of the same law; but we must postpone a discussion of it to some future occasion.

There is then no question that Miss Baker's criticism of Cohn's procedure, while logically valid, is not substantiated by experimental facts. In searching for a reason why there should be no difference in the degree of pleasantness of color-pairs whether the individual colors are shown first or last, we must consider that in either case the subjects themselves are not aware of a gradation of the affective tone of the single colors beyond the possible knowledge of the colors which they like best or least. This knowledge may have helped certain subjects in certain cases, especially where a decision between two pairs could not be made otherwise. For this reason we have asked our 32 additional subjects after the experiment whether they had been influenced by such a consideration; and nearly all of them admitted that sometimes this had helped them to choose between two pairs.[6] Nevertheless this more or less frequent, conscious and deliberate use of the affective tone of one constituent for the determination of the affective tone of the combination can not account for the consistency and uniformity of the various kinds of combinations into whose choice such deliberations did not enter at all. It could, for instance, not be responsible for the fact that the most-least and the medium-medium pleasant combinations are practically half way between both extremes and almost identical with each other. In the case of the second set of subjects we must also assume that as a matter of fact nearly everybody knows what his favorite colors are; and that, as has been shown by various investigators, one's preferred colors remain practically constant over long periods of time, so that they are not likely to be very much more

[6] As a matter of fact this criterion has also been employed with full deliberation by the child in his recent observations.

enhanced by a single previous exposure. We have also found by questioning our subjects after the experiment as to how many different colors had been employed in it, that they were rather confused by so many different combinations following each other in fairly rapid succession. Hence nearly all of them guessed that there had been more than ten individual colors. There seems then to be no other reason for the lack of a difference between the two sets of subjects than the fact that both groups chose those color-pairs more frequently whose constituents had the higher degrees of pleasantness.

V. Elimination of Possible Irrelevant Influences

We may now consider the question whether there were any other factors which showed a consistent influence upon the affective tone of single and paired colors. In the first place, we may mention, as a parallel to Miss Baker's results with a Greek subject who disliked the combinations with a green because it happened to be the color of the Turkish turban, the case of one of our women subjects of Irish descent who for a similar reason disliked orange and all its combinations. The remarkable thing is only that among our 122 subjects we did not find more such cases, since Miss Baker found one among her 30 subjects.

Another possible factor is the complementariness of colors. The influence of the pure complementary relation could show itself in our results only in the case of the B-Y pair. Among our first set of subjects there were 19 men and 19 women who selected this pair with a frequency greater than 14; but when these two colors were presented separately, 28 of these 38 subjects showed "individual frequencies" which when added together gave values from 7 to 11, while 5 of the other 10 subjects were "reversals"; hence we feel justified in stating that the complementariness of B and Y had no influence upon the affective value of their combination.

With regard to "near-complementaries," for whose combinations Kirschmann and some of his students claimed a degree of pleasantness greater than that of any other combinations, we have among our stimuli the following examples: R-G, R-Bg, G-P, O-P, and O-B. To what extent these pairs

are more pleasant than other combinations, irrespective of the degree of pleasantness of their constituents, may best be seen from their comparison with corresponding results obtained from those six extreme non-complementaries which in our series were adjacent to each other, namely, R-O, O-Y, Y-G, G-Bg, Bg-B, and B-P, and which thus represent the greatest opposition to complementariness. Our five "near-complementary" pairs were preferred 90 times with a frequency between 14.5 and 20; and in 66 cases (or 72 per cent) the added "individual frequencies" of their constituents totalled between 7 and 11. Likewise the six "adjacent" pairs occurred 111 times with a frequency above 14, and in 74 cases (or only 67 per cent) their constituents totalled 7 or more. This shows that the "near-complementaries" are slightly less frequently chosen as very pleasant pairs than are the "adjacent" pairs. The difference is however so small as to be negligible; and our results fail to confirm those of Miss Baker as well as those of Cohn who had argued in favor of the greater pleasantness of the complementaries.

We have not been able to discover the influence of any other single factor upon the affective tone of our results, although the large size of the mean variations in many of our groups seem to point to the presence of such factors. A few chance remarks from some of our subjects after the experiment indicated the nature of some of these factors. A single case may suffice as an illustration: one girl remarked that she had chosen the O-B pair very often because her art-teacher had once told her that this makes a very artistic combination. We may also point out here that the mean variations in Table I, especially those by the method of "constant extremes," show a remarkable constancy in magnitude, no matter to what average they belong. The writer can at present offer no satisfactory explanation of this phenomenon.

VI. Measurements of Deviations from the Ideal Standard

We have, however, been able, by the following method, to indicate or measure to what extent any given color-pair,

or subject, or groups of either, approximate the general law. For this purpose we have constructed an ideal standard or norm on the assumption that a color-pair whose constituents had "individual frequencies" of 6 and 5 respectively should, if no other influences are operative, be more frequent than any other pair, that is, it should have the absolute frequency of 20. We may then compute how far any given color-pair A-B deviates from this norm by adding the "individual frequencies" A+B, multiplying the sum by 20, and dividing the product by 11. The deviations may be expressed in percentages with the standard representing 100 per cent. It is possible for a given color-pair to deviate from such a standard in the positive direction as well as in the negative direction, because pairs of individually less than maximal pleasantness may as pairs have maximal pleasantness, or at least may have a frequency greater than the one corresponding to the sum of their "individual frequencies."

We have in this way computed the percentages of the deviations for each color-pair with our first set of subjects, with the "typical" subjects, and with the 41 "typical" men and 38 "typical" women separately; and the results are given in Table II. The color-pairs in column 1 are given in the order from greatest to smallest positive deviation and then from smallest to greatest negative deviation, from the ideal standard as calculated by the above method. In determining the average deviations of columns 2, 3, 4, and 5, row 24, the positive and negative signs have been taken into account.

Beginning with the pair Y-B, which with all 90 subjects shows the greatest positive deviation, we find that with the 79 "typical" subjects this pair drops to fifth place, so that it has earned its first rank in column 2 through the overwhelming preferences by the "reversals" and also to some extent by the 38 "typical" women, as is shown in column 5, row 3. If the four different ranks which it has obtained in columns 2, 3, 4, and 5, are averaged, we find that Y-B receives an Average-Position of 4.25; and comparing this with the average-positions of the other pairs, we find that its final Average-Rank is 3. The absolute frequencies with which Y-B has been chosen by the 45 men and 45 women

TABLE II

	1	2		3		4		5		6		7		8		9	
1		Percentages of Deviations from Ideal Standard										Absolute Frequencies					
2	Color-pairs	90 Ss	R'k	79 Typ.	R'k	41 Typ. M.	R'k	38 Typ. W.	R'k	Av. Pos.	Av. R'k	45 Men	R'k	45 Women	R'k	Diff. in Freq.	R'k
3	Y-B	+6.10	1	+1.39	5	—2.26	7	+5.08	4	4.25	3	537	4	531.5	5	5.5	1
4	G-BG	+5.53	2	+2.48	4	+4.28	3	+.94	6	3.75	2	494	7	554.5	3	50.5	4
5	O-BG	+5.03	3	+ .89	6	—10.00	15	+9.18	1	6.25	6	316.5	21	519	6	202.5	15
6	R-BG	+4.78	4	+3.74	1	+.45	4	+6.69	3	3.00	1	448.5	10	564	1	115.5	9
7	O-Y	+3.71	5	+3.49	2	—3.59	10	+9.14	2	4.75	5	335.5	20	516	8	180.5	12
8	O-R	+1.83	6	+2.53	3	+11.08	1	—4.89	8	4.50	4	450	9	469.5	10	19.5	1
9	Y-BG	+1.81	7	—3.18	9	—9.30	14	+1.66	5	8.75	9	358	19	541.5	4	183.5	15
10	R-G	—2.49	8	—3.40	10	+1.72	6	—6.27	10	8.50	8	518	6	556.5	2	38.5	4
11	O-G	—4.51	9	—8.23	13	—2.50	8	—13.51	15	11.25	12	428	14	465	12	41	2
12	R-Y	—5.50	10	—.35	7	+.24	5	—4.87	9	7.75	7	439.5	12	498	9	58.5	3
13	BG-P	—5.67	11	—6.08	11	—9.06	12	—2.96	7	10.25	11	440	11	466.5	11	26.5	0
14	G-P	—6.07	12	—17.64	17	—9.26	13	—19.91	17	14.75	15	536	5	427	13	109	8
15	G-B	—9.13	13	—11.76	14	—4.10	11	—24.28	19	14.25	14	618	1	398	16	220	15
16	R-B	—10.73	14½	—7.73	12	—3.56	9	—13.27	14	12.37	13	570	2	403	14	163	12
17	Y-G	—10.73	14½	—14.71	16	—18.63	20	—10.53	11	15.37	16	394	16	516.5	7	124.5	9
18	O-B	—12.50	16	—2.28	8	+4.66	2	—11.53	13	9.75	10	416	15	389	18	27	3
19	BG-B	—12.88	17	—13.89	15	—16.43	18	—10.83	12	15.50	17	455.5	8	400	15	55.5	7
20	Y-P	—20.48	18	—18.66	18	—19.59	21	—17.64	16	18.25	18	387.5	17	395.5	17	8	0
21	O-P	—22.55	19	—18.70	19	—12.33	16	—25.79	20	18.50	19	385.5	18	310	21	75.5	3
22	B-P	—22.69	20	—19.77	20	—14.83	17	—21.84	18	18.75	20	541	3	297.5	21	244.5	18
23	R-P	—30.75	21	—26.25	21	—18.59	19	—33.02	21	20.50	21	428.5	13	312.5	19	116	6
24	Average	—7.04		—7.53		—6.43		—9.00		-Total		9450.0		9450.0			

separately are shown in columns 7 and 8 together with the ranks according to these frequencies. Finally, in column 9 the absolute differences between these two frequencies and ranks are indicated. A detailed inspection of this table will bring out some very interesting facts but no very striking uniformities or consistencies, except some sex-differences to which we shall return in a later part of this paper.

When all 90 subjects are taken into account, the percentages of deviations from the ideal standard may be divided into four groups, as indicated by pairs 1-7, 8-12, 13-17, and 18-21. The first group includes the positive deviations, or "exaggerations" of our law, as we may call it, since there seems to be no other consistent factor on the basis of which these exaggerations might be explained. The second group deviates negatively by less than the average-deviation, and again there is seemingly no consistent factor involved. The deviations of the third group are less than twice the size of the average deviation and contain four of the six pairs with blue. The last group contains only four pairs with purple and exceeds the average from three to more than four times. These same four pairs retain their low ranks in all the other columns, which goes to show that color-pairs with purple receive a consistently lower pleasantness than that to which, according to our law, they are entitled. Again we have no satisfactory explanation to offer for the peculiarity. No striking differences are revealed by a comparison of columns 2 and 3, except perhaps that the pair G-P joins here the other four pairs with purple.

VII. Sex and Age Differences

Certain characteristic sex-differences are, however, apparent, when we compare the "typical" men and women according to columns 4 and 5. The greatest disagreement between them is with regard to the pair O-Bg, which the men seem to dislike, while the women give it first rank. The same fact is also apparent from the absolute frequencies of this pair. The difference in this case is not due to the "individual frequencies" of the components, because there is only a moderate difference between them, as is shown in Table III,

row 3, col. 5 and 7 and 14 and 16, the "typical" men choosing O 76 times as against 96.5 by the women, and Bg is chosen by the men 91 times as against 119 times by the women; this difference is no larger than with Y, and it is considerably smaller than with B and P.

The next largest difference between the "typical" men and the "typical" women is manifested in the pair O-B, although here the absolute frequencies show no confirmation because the "reversals" have obliterated the fact here, as may be inferred from a comparison of the ranks of O-B in columns 2 and 3. Other differences betwen the "typical" men and the "typical" women occur with the pairs O-Y, Y-Bg, O-G, and Y-G, while on the basis of absolute frequencies for all men and all women the pairs R-B and B-P should also be added. The latter two pairs involve B which shows a great difference in its "individual frequency" for men and for women, as may be seen from Table III, row 5, columns 17 and 19.

The question as to the extent to which our results show any uniformity with regard to the age of the subjects has been attacked by us so far only in connection with the single colors; but if characteristic differences should be later found to exist also with the color-pairs, we shall publish a separate Note about them.

A summary of the results obtained with the single colors is given in Table III. Here the absolute frequencies of the single colors are classified according to three groups of subjects, according to age-groups, and according to the three degrees of pleasantness as based on the method of "varying extremes" and on that of "constant extremes." In all of these main divisions we have retained the men-women subdivision in order to detect any further sex-differences which might be present.

First as to age-groups, rows 9, 10, and 11. We divided our 90 subjects into those below 20 years, those between 20 and 25, and those above 25 years, and determined the percentages by which each of these smaller groups exceeded or fell below the standard set by the whole group of 45 men or 45 women. These differences in percentages are

TABLE III

		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
1	Absolute Frequencies	Red			Orange			Yellow			Green			Bluegreen			Blue			Purple		
2	Single colors	M.	Both	F.	M.	Both	F.	M.	Both	F.	M.	Both	F.	M.	Both	F.	M.	Both	F.	M.	Both	F.
3	79 "Typ." Ss	125.5		127.5	76.0		96.5	100.0		122.0	144.0		144.5	91.0		119.0	177.5		94.5	147.0		104.0
4			253.0			172.5			222.0			288.5			200.0			272			251.0	
5	90 Subjects	138.0		157.5	88.0		113.5	116.0		134.0	154.0		156.5	102.0		134.0	188.5		115.5	158.5		134.0
6			295.5			201.0			250.0			310.5			236.0			304.0			292.5	
7	122 Subjects	200.0		200.5	115.0		147.5	159.0		183.0	215.0		221.5	121.0		200.0	256.5		139.5	214.5		189.0
8			400.5			262.5			342.0			437.5			321.0			396.0			403.5	
9	Age groups	—20	—25	25+	—20	—25	25+	—20	—25	25+	—20	—25	25+	—20	—25	25+	—20	—25	25+	—20	—25	25+
10	Diff. in % ...... 45 M.	+2	0	—2	—4	+5	—1	—3	+2	+1	+3	—2	—1	—1	+3	—2	0	+2	—2	—1	+2	—1
11	From 45Ss ...... 45 F.	—4	+5	—1	+4	—9	+5	+8	—3	—5	+9	+2	—11	—10	+3	+7	+11	—2	—9	—3	—1	+4
12	Degrees of Pl	M.	Md.	L.	M.	Md.	L.	M.	Md.	L.	M.	Md.	L.	M.	Md.	L.	M.	Md.	L.	M.	Md.	L.
13	90 Ss ...... M.	4	36	5	0	31	14	5	29	11	9	32	4	7	22	16	24	17	4	13	27	5
14	Var. Extr ...... F.	8	36	1	4	33	8	6	31	8	13	28	4	17	15	13	8	23	14	7	30	8
15	M. + F.	12	72	6	4	64	22	11	60	18	22	60	8	24	37	29	32	40	18	20	57	13
16	Degrees of Pl	6, 5; 4, 3, 2; 1, 0			6, 5; 4, 3, 2; 1, 0			6, 5; 4, 3, 2; 1, 0			6, 5; 4, 3, 2; 1, 0			6, 5; 4, 3, 2; 1, 0			6, 5; 4, 3, 2; 1, 0			6, 5; 4, 3, 2; 1, 0		
17	90 Ss ...... M.	8	30	7	0	28	17	4	28	13	13	27	5	10	19	16	24	17	4	13	28	4
18	Const. Extr ...... F.	12	27	6	4	31	10	10	24	11	15	23	7	16	13	16	11	16	18	9	24	12
19	M. + F.	20	57	13	4	59	27	14	52	24	28	50	12	26	32	32	35	33	22	22	52	16
20	122 Ss ...... M.	13	40	8	1	35	25	6	38	17	19	36	6	10	24	27	31	25	5	18	38	5
21	F.	13	39	9	5	39	17	16	29	16	23	29	9	25	18	18	11	23	27	12	35	14
22	M. + F.	26	79	17	6	74	42	22	67	33	42	65	15	35	42	45	42	48	32	30	73	19

indicated by the plus or minus signs in rows 10 and 11. In the case of the men there is almost no appreciable age-difference,—the only minor exception being O, row 10, columns 5, 6, and 7, which shows that the men below 20 differ from the male norm by minus 4, and the men between 20 and 25 differ from it by plus 5. On the other hand, with the women the age-differences are much more marked in every color except perhaps P and R, as is indicated by row 11. Here the younger group prefers Y, G, and B by 8, 9, and 11 per cent more, and Bg by 10 per cent less, than the female norm; the medium age-group shows a decided difference of —9% only with O, while the older group is practically the reverse of the younger group. Whether with the women this larger difference among the age-groups was merely accidental or not, we were unable to determine by means of the additional 16 women, because they included no persons over 24 years of age.

This possibility of checking the results was, however, realised in the case of the other large divisions. If we study first the absolute frequencies of the individual colors we find that, without regard to sex, the color G is most frequently preferred by all three groups of subjects,—the 79 "typicals," the original 90, and the 122 forming the grand total; this certainly indicates that the preference for G is not accidental. An analogous state of affairs exists for O as the least frequently preferred color. But the analogy holds also with all the other colors for all three divisions of subjects, with the sole exception of B and P, which for the 122 subjects interchange places. The order of these seven colors according to their absolute frequencies for both sexes together is then: green, blue, (purple), red, purple (blue), yellow, bluegreen, and orange.

For the men alone, the order from most to least pleasant is changed to the following which holds for all three subdivisions: blue, purple, green, red, yellow, bluegreen, and orange. We do not find here any confirmation of the current belief that men like red better than any other color. This, however, may be due to the fact that our colors were pre-

sented in isolation; for certain purposes, such as interior decoration, men may show such a preference.

In the case of the women the orders in the three subdivisions are much more variable, at least with regard to the colors which occupy the three middle ranks. For the first rank G has a very strong competitor in R; while Y, P, and Bg are almost tied for the third, fourth and fifth places; and O and B are just as nearly equal and occupy sixth and last places respectively.

In order to make the comparison easier for the reader, the three ranks for all 122 subjects are here given in parallel

TABLE IV

ORDER OF PREFERENCES OF SINGLE COLORS

Rank	61 *Men*	122 *Men and Women*	61 *Women*
highest	Blue	Green	Green
intermediate	Purple	Purple	Red
	Green	Red	Bluegreen
	Red	Blue	Purple
	Yellow	Yellow	Yellow
	Bluegreen	Bluegreen	Orange
lowest	Orange	Orange	Blue

columns. It must be emphasized again that in certain instances the differences between successive colors in these serial orders are so slight that the findings from ten additional subjects might reverse their relative positions. Hence we have indicated the larger differences by horizontal lines. It is interesting to note that men and women are absolutely opposed to each other in regard to blue; and that they disagree strikingly, though not to such a marked degree, with regard to bluegreen. Further light is thrown upon these sex-differences when the absolute frequencies of the three degrees of pleasantness are taken into consideration, which is done in the lower half of Table III, rows 12 to 22.

The actual frequencies of the first set of subjects are shown as computed by the two methods, that of "varying extremes" in rows 12 to 15, and that of "constant extremes" in rows

16 to 19. A comparison of these rows will reveal no characteristic differences; so that we may disregard the former and refer only to the latter. The preponderance of B with the men is found to be due to the great number of the "individual frequencies" 6 and 5 (column 17, rows 5 and 7) and to the rare occurrences of the "individual frequencies" 0 and 1. Exactly the opposite condition is true in case of the women. In regard to O, the men and the women agree in most frequently assigning to it the lowest degrees of pleasantness, as columns 5, 6, and 7 show. The men and the women also agree, contrary to popular opinion, in the number of frequencies which they manifest for the varying degrees of pleasantness of R. The other colors reveal no striking sex-differences or any other characteristic uniformities, except that in its sex-contrast Bg resembles B, but to a lesser degree.

On the whole we may say that the men show more decided differences between their highest and their lowest degrees of pleasantness than do the women, both with regard to the single colors and to the color-pairs, at least as far as is indicated by the absolute frequency differences between the first and the twenty-first pair. And another general sex-difference may be pointed out in this connection, namely the fact that the men adhere more closely than the women to the general law of the summation of the affective tone in color-pairs, which has a bearing upon the frequent assertion that women are more sophisticated in the matter of color, because color plays a more prominent rôle in the every-day affairs of women.

VIII. Summary and Conclusions

(1) In the present investigation the emphasis was laid upon the behavior of the affections in color-combinations as compared with the affective tone of the individual constituents. The experimental work, while involving the familiar method of "paired comparison," represents a novel attack upon the problems of the affective life which promises to be more fruitful than previous methods of attack.

(2) The results obtained from 122 subjects giving 25,620 preferences of color-pairs as compared with their 2,562 preferences of the single colors constituting these pairs prove

beyond doubt the operation of a general law of the summation of pleasant affections of single colors in color-pairs which may be formulated as follows: *the greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination.*

(3) The relation of exact complementariness as well as the relation of "near-complementariness" in the constituents of a color-pair have no uniformly consistent influence upon the degree of pleasantness of the color-pair.

(4) Color-pairs which contain purple as one member show the greatest and most uniform deviation from an ideal standard which is based on the assumption that there are no other influences operative in the choice of color-pairs than the degrees of pleasantness of the individual components. Certain other color-pairs manifest what for lack of another principle of explanation may be called an exaggeration of the law of the summation of affections.

(5) A number of general and specific sex-differences were found. Among the *general* sex-differences the following are most marked: (a) Men show more decided differences between their highest and their lowest degrees of pleasantness than women, with regard to both single and paired colors; (b) Among men a slightly closer adherence to the ideal standard is detected than among women.

(6) The more prominent *specific* sex-differences are: (a) Women prefer the pair O-Bg much more frequently than men; and the difference is only slightly less in the case of O-B. These two differences can not be explained on the basis of the degree of pleasantness of the individual members of the pairs; (b) With regard to the degree of pleasantness of the single colors, women show decided age-differences; the difference between the groups below twenty and above twenty-five years of age is especially prominent; (c) For other sex-differences in the rank of colors according to preference we must refer to Table IV, where the most decided contrast appears with color B, which was preferred most frequently by the men, but least frequently by the women; (d) The highest and the lowest ranks of the single colors are determined chiefly by the absolute frequencies of the higher and lower degrees of pleasantness.

A NOTE ON RECOGNITION

By Helen M. Clarke,
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The explanation of recognition is by no means a settled problem in psychology, and yet it is by no means a new problem. One is surprised at the number of studies that one finds, and the classification of theories. There is room for only the merest reference to these here. Perhaps the oldest theory is that recognition consists of the conscious comparison of a percept with a memory image; another finds the differentia of the process in the feeling of familiarity; a third in a quality of knownness which cannot be further interrogated; a fourth in the arousal of associates; and a fifth in motor adjustment. There are many explanations which may not come exactly under any of these heads, but are more or less closely related to them. One is tempted to guess that the first named type of theory is a deduction from the usual definition of recognition without appeal to introspection. It is mere common sense to suppose that recognition must be the comparison of a percept with a memory image. But as a matter of fact the reported cases of such comparison are so few as to be negligible. The presence of the feeling of familiarity can readily be verified in many cases but not in all. To accept the third view is to fall back on the unknowable and give up the problem. The association theory is based on many observations, but the surer and easier recognition is, the less are associations to be observed. The motor adjustment theory is very attractive, but its limits are too narrow.

In spite of the bewildering number of conflicting views, there are some facts which may be assumed with little fear of contradiction from any one who has read the investigations of the last ten years or more. Among these are the following:

The conscious contents constituting recognition gradually decrease as the percept becomes more familiar and recognition becomes easier and more accurate. When this change has been

carried very far we have an automatic and almost unconscious process called recognition. The details of this process of shortening have been observed. Titchener[1] compares it to the change from impulsive to reflex action. The affective part of recognition is weakened and gives place to indifference, the organic sensations are disregarded, we take the thing for granted, and recognition becomes cognition. We may recognize ideas as well as percepts;[2] and even an idea of imagination, once entertained, may be recognized on later appearance. Whether or not perceptual and ideational recognition are the same is not so well agreed upon. The apprehension of the new is a positive experience[3] and not a mere absence of recognition. Recognition, like any other experience, is influenced somewhat by the imaginal type and other characteristics of the individual. Recognition comes under the sway of the *Aufgabe*.[4] The distinction between correct recognition and memory on the one hand and illusion of either on the other, is secondary and practical rather than psychological. If this were not the case, the study of illusions of perception could not throw light, as it has done, on normal perception.

My own view of recognition is based on a long series of experiments performed in the Cornell laboratory and already reported on.[5] In the main series were used letters from the New York point alphabet for the blind. The observers received the stimulus tactually and reacted by pressing a key when it was recognized. The reaction times were kept. The main conclusion drawn at the time is one which might be

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[1] E. B. Titchener, *An Outline of Psychology*. 1910, p. 70.

[2] R. B. Owen, Recognition: A Logical and Experimental Study. *Psychological Monographs*, XX (No. 86), 1915; W. B. Pillsbury, *Essentials of Psychology*. 1911, p. 207.

[3] E. B. Titchener, *A Text-book of Psychology*. 1910, p. 410; E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-Processes*. 1909, p. 179; M. W. Calkins, *An Introduction to Psychology*, 1908, p. 199.

[4] N. Ach, *Ueber die Willenstätigkeit und das Denken*. 1905; E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-Processes*. 1909, pp. 120ff.

[5] Helen M. Clarke, Conscious Attitudes. *American Journal of Psychology*, XXII, 1911, pp. 214-249.

stated in the negative way: that recognition is not an unanalysable content as some have claimed; that in every case the state of consciousness either can be analysed explicitly into sensational and affective elements or can be traced backward through the process of shortening to a state which can be so analysed.

So much for what recognition is not. Are there grounds here for even a hint as to what it is? Further study of the reports bring out the following facts: The imaginal type of the observer shows itself clearly in the recognitive consciousness, and especially in the early stages of it when it is rich in content. One observer visualizes the point letter itself and the written letter corresponding, and is aided in recognition by association with certain colors. Another describes the letter in words. Another frequently reports a visual image of a certain location in his scheme of the alphabet, and at the same time organic and kinaesthetic sensations, feeling of familiarity, and pleasantness. But along with these differences there are some striking similarities. All alike show in marked degree the dropping out process and the approach to affective neutrality, running parallel with the increased accuracy and speed of recognition. But not only so; all show a change in one definite direction. As the images and feelings drop out, recognition, for all the observers, tends toward the one type in which the perception of the letter calls up its name and nothing else is present in consciousness. This report is made much oftener than any other, and much oftener in the second half of any series than in the first half. Moreover, toward the last of the series even this disappears and the touching of the letter sets off the reaction without even the appearance of the name. More and more frequently occur such reports as the following: The name and the pressing of the key occurred at the same time. The name came to consciousness after the reaction. I touched the letter and reacted automatically.

What, if anything, does this show? Not that all recognition passes through a stage in which it is mediated by a mere name, nor that all recognition becomes a motor reaction; but that given the present *Aufgabe,* recognition for the various

observers tends to approximate this one type through practice. I should say *Aufgaben* for there are two—one implicit and one explicit. The normal reaction to a word or letter is to pronounce it. We may be said to act under a more or less permanent *Aufgabe* to read what is written. It may easily be supposed that some other form of experiment would call out a different type of reaction. But in addition to this natural reaction, the experiment requires the formation of a closer and closer association between the felt letter and another kind of reaction—the pressing of the key. The indications are that if the experiment had been carried far enough the habit would have been completely formed, and the pressing of the key would have been the most important thing in consciousness, if not all that was distinguishable. Fortunately we need not merely speculate on this. There have been careful studies of such processes as that of learning to use a typewriter.[6] They show not only the shortening process, but a final stage in which letters and whole words and phrases are reacted to as wholes mechanically. The recognition comes to consist in the motor reaction. This is the experience of the pianist, the skilled performer on any instrument, the expert in the use of tools. This is probably true of the animal which reacts characteristically to a familiar experience. We say that a dog recognizes his master and his home; he gives every evidence of recognition, and this evidence is a motor reaction. He may and probably does have feeling and organic sensations, but we are not justified in assuming ideational thought or even the mere presence of memory images.[7]

Introspection finds pleasantness and the feeling of familiarity in many cases of recognition, but it also traces their gradual loss.

Calkins[8] and others have pointed out that the new is recognized as new, just as the old is recognized as old. Both are

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[6] W. F. Book, The Psychology of Skill, with Special Reference to its Acquisition in Typewriting. *University of Montana Studies in Psychology,* I, 1908, pp. 188.

[7] C. H. Judd, What is Perception? *Journal of Philosophy, Psychology and Scientific Methods,* VI, 1909, p. 36.

[8] M. W. Calkins, *An Introduction to Psychology.* 1908, p. 258.

positive experiences and neither is the mere absence of the other. Titchener says: Every sensory stimulus of moderate intensity arouses a widespread organic reaction.[9]. The feeling of familiarity has been analyzed into feeling and organic sensations. It is agreed that this feeling diminishes as the experience is repeated. There seem to be two possibilities then. The feeling of familiarity may be absent in the new, may arise perhaps suddenly when the new becomes slightly familiar, and may gradually disappear as familiarity—not the feeling of familiarity—increases and recognition approaches cognition. Can this sequence be verified by experiment? Is it true that the new lacks the organic reaction and affective tone and can we find by experiments the exact degree of knownness at which this arises? If so, this should appear in the experiments with point letters. They show, on the contrary, that the organic and affective consciousness is most rich at the very beginning and decreases gradually. There is a logical objection as well as the experimental. What is the new? It is admitted that recognition may be of varying degrees of definiteness. We recognize a thing as belonging to a class before we identify it individually. Moreover, a false recognition is psychologically recognition. We live under a constant *Aufgabe* to recognize, to classify, and we do so consciously or unconsciously. Most of the mistakes of children can be attributed to the working of this principle. It may be that the absolutely new, which is not recognized even vaguely and generally, may lack any feeling of familiarity. If it ever occurred it would almost certainly have so widespread and intense an organic reaction of some kind that any feebler feelings would be hard to attend to. But the wholly unclassifiable practically never occurs; and the new in the ordinary sense, being not wholly new, has the feeling of familiarity. It has been often shown that it is the vaguely familiar, that which baffles and refuses to be made definite, which calls out the strongest feeling of familiarity.

But if the motor and organic reaction play an important but decreasing part in recognition, do they exhaust the recognitive consciousness? Is imagery merely incidental or may it play

[9] E. B. Titchener, *A Text-book of Psychology.* 1910, p. 407.

an essential part? If we reply in the negative and rely upon motor reaction alone, it may be that we have merely examined a type of recognition in which the *Aufgabe* is to react and in which therefore the motor reaction plays an important part. How do we recognize a word? If it is a substantive and signifies anything picturable, a visualizer may identify it at first by means of visual images. Any kind of imagery may be used or several kinds at once. But the imagery tends to wear down to the purely verbal and in many cases it is impossible to find anything in consciousness but the word itself.[10] In reading, the normal reaction to a word is its mental pronunciation, and recognition approaches this as a limit.

If I should hazard the attempt to reduce these thoughts within the limits of one brief statement, it would be something like this: Recognition is the total reaction to the stimulus or idea to be recognized. With the relatively unfamiliar it is complex, largely organic, affectively toned, and determined partly by the type of the observer. As the stimulus becomes more familiar, it approaches simplicity and affective neutrality, the imaginal type depending on the nature of the *Aufgabe* to recognize; hence on the part that the particular stimulus plays in common experience.

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[10] C. H. Judd. *Ibid*, p. 36.

MEANING AND PROCESS AS DISTINGUISHED BY THE REACTION METHOD

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I. Introduction

We are concerned, in this paper, with certain aspects of the relation of logic to psychology, of meaning to content-process. The psychological point of orientation is to be found in the context-theory of meaning.[1] This theory is one of those general formulations, well known to science, which derive from a comprehensive survey of facts, and which look to experiment for verification of their schema and completion of their detail. It is of the essence of the theory that meaning, like everything else, may be regarded from different points of view; meaning is biological when regarded as the organism's reaction upon a situation, *i. e.*, upon a complex of stimuli to which it has become adapted; meaning is logical[2] when regarded as significance, as interpretation, as knowl-

[1] For general statements of the theory, see E. B. Titchener, *Experimental Psychology of the Thought-Processes,* 1909, pp. 174ff.; *Text-Book of Psychology,* 1910, pp. 367ff; *Beginner's Psychology,* 1915, pp. 26ff, 118ff. We hope later to publish a detailed statement of the theory and an account of its precursors in the history of psychology.

[2] We use the term 'logical' in its broad sense. The meanings with which we are particularly concerned are not those of systematic logic; they are rather individual or pre-logical meanings.

edge; it is psychological when regarded as an item of mental experience, a group of content-processes. Meaning, then, is biological response, or logical signification, or psychological experience, as the case may be. Psychologically considered it is in the first instance, according to the context-theory, the complex of processes accruing to the given process through the situation in which the organism finds itself. Like all such formations, this complex is subject to the law of mental growth and decay; content-processes which at first are vivid and complete gradually become obscure and fragmentary, and ultimately may drop out of consciousness altogether. Thenceforth, in explanatory psychology, logical meaning is correlated with brain-habit. The problem which the context-theory of meaning sets for psychology, therefore, is in general twofold: descriptive, for the observable content-process coördinate with the logical meaning must be described; and genetic, for content-processes which are no longer observable must be rediscovered. The experimental part-problems are both numerous and varied. We may, for example, require a characterization of logical and psychological attitudes in terms intelligible both to logic and to psychology; we may ask whether a single situation has a single meaning or whether it may have several meanings and, if so, how these meanings differ; we may inquire specifically into the conditions under which meaning occurs; we may investigate the temporal relation between a logical meaning and its correlated content-process; we may seek evidence for or against the concept of brain-habit in other fields of systematic psychology, *e. g.*, in recognition, memory, action; as physiological psychologists we may attempt an adequate account of brain-habit in physiological terms. Further problems will, no doubt, emerge as experiment proceeds.

II. *The Experiment:* 1. *Problem.* In planning our experiment we had two of the above mentioned problems in view. We hoped (1) to obtain characterizations of the logical and psychological attitudes, and (2) to make a preliminary investigation of the temporal relation between logical meaning and correlated content-process. As the work went on we found that our observers were responding to the experimental

situation with several types of meaning. We therefore carried out further experiments, with change of method, in the hope of bringing these kinds of meaning into bolder relief.

2. *Method, Apparatus and Observers.* We employed a method of reaction; stimuli were presented to the observer, and reactions were taken separately to meanings and to visual imagery. The stimuli consisted of twenty-nine printed words, selected from ethnological and technical vocabularies, whose meanings were at the outset unknown to our observers.[3] We then furnished meanings by aid of pen-and-ink drawings of the objects represented by the words. The observers were instructed so far as possible to inhibit verbal and kinaesthetic learning. After a few repetitions of words and pictures, the learning was continued with words alone in the form of a practice series with reactions to the meanings; if a meaning could not be recalled, the picture representing the particular word was shown. As soon as the observer was able to go through the series without being shown a single picture, the experiment proper began. The stimuli were arranged in three groups of 9, 10 and 10, and the order of presentation of the groups was changed from series to series.[4] The general instructions were as follows: "You will be shown a word: you are to react when you have grasped the meaning of the word, *or* when you have a visual image. Before the presentation of every word you will be told whether you are in its case to 'react to meaning' or to 'react to image.'" In the first series the alternative instructions were given in irregular order, there being in all 15 instructions for image and 14 for meaning; in the second series the instruction for every stimulus was reversed; in the third, the order was the same as that of the first; in the fourth, it was again reversed,

[3] The stimuli arranged in groups as used in the experiments with D and R were: (A) burin, peruke, brob, mino, parbuckle, costrel, cresset, martlet, mace. (B) pavon, besague, chank, ampyx, mambrino, araba, chela, corbel, dagon, argali; (C) hippogriff, kago, hubblebubble, jougs, koto, heaume, sabbaton, aquaemanale, acontium, aplustre. The pictures employed were pen and ink drawings of the illustrations of the words as found in the Century Dictionary.

[4] There were originally 30 words, but one of these proved on trial to be vaguely familiar, and was accordingly discarded.

and so on. Reports were not asked for during the regular series, although after a reaction to meaning the observers frequently volunteered a report of process. Series with report were, however, occasionally taken at the close of the regular series.

The apparatus consisted of a lip key, a tachistoscope, and a Hipp chronoscope. Before every experiment the chronoscope was tested by means of the large Wundt control hammer set at 150σ, and the strength of current was adjusted until the mean variation was equal to (or less than) 1.5σ. Previous tests of the same instrument had shown that when adjusted for constancy at 150σ, it was also reliable for intervals as large as 1750σ.[5] The observers were F. L. Dimmick (D), and J. G. Rich (R), graduate students in psychology.

3. *The Quantitative Results.* The gross results for D and R, consisting of the total reaction-times both to meaning and to image, together with the differences and the probable significance of these differences, are shown in Table I.

TABLE I

AVERAGE AND PROBABLE CORRECTNESS FOR TOTAL NUMBER OF CASES[6]

Observers	D		R	
	Image	Meaning	Image	Meaning
Average	1381.4	1481.5	669.6	582.9
Number of cases	162	164	153	157
M. V.	325	470	127	121
P.E.	274	397	107	102
P.E$_M$	21.5	31.0	8.6	8.1
D	—100.1		+86.7	
P.E$_D$	37.7		11.8	
Probable correctness	96%		100%	

D = difference between total times for meaning and total times for image. Plus = image-time is greater than meaning-time.

[5] W. S. Foster, On the Perseverative Tendency. *American Journal of Psychology,* XXV., 1914, p. 408.

[6] For the method of determining probable correctness of the difference, see E. G. Boring, The Number of Observations upon which a Limen may be Based, *American Journal of Psychology,* XXVII, 1916, pp. 315ff.

It will be noticed that D gives reaction-times to meaning that are on the average longer than those to image, whereas R gives reaction-times to meaning that are shorter than those to image. The probable correctness of the difference between the total averages is found to be 96% and 100% respectively.

Let us, however, turn to details. If we take the total reaction times of the series in order, as in Table II, facts

TABLE II

AVERAGE REACTION-TIMES BY SERIES

Series	Meaning	M.V.	Image	M.V.	Difference
Observer D					
1	1445	285	1215	116	—230
2	1507	585	1419	219	— 88
*3	2286	713	1442	357	—844
4	1326	269	2398	1353	+1072
5	2140	1341	1230	242	—910
6	1381	205	1310	156	— 71
7	1318	175	1212	135	—107
8	1137	175	1271	211	+134
9	1666	536	1331	232	—335
10	1286	232	1352	299	+ 66
11	1315	140	1242	87	— 73
12	1158	75	1315	193	+157
Average	1481.5	470	1381.4	325	—100.1
Observer R					
1	736	92	874	178	+138
2	702	80	798	175	+ 96
3	642	62	715	73	+ 73
4	691	143	720	110	+ 29
5	593	76	603	62	+ 9
6	489	70	575	66	+ 86
7	514	66	585	69	+ 71
8	391	33	526	69	+135
*9	641	112	954	287	+313
10	540	74	589	94	+ 49
11	469	81	591	96	+122
Average	582.9	121	669.6	127	+ 86.7

* Interval of three weeks.

which result from the serial nature of our experiment become apparent. For R, the image-times at first grow shorter and then tend to become constant; the average of the last six series (the 9th is disregarded since it occurred after an interval

of three weeks) is 578 ± 18.5. The meaning-times tend to become shorter, but with less regularity; the average of the meaning-times of the same six series is 499 ± 50. For D, on the other hand, these relations are not obvious. There is, it is true, a slight tendency for the meaning-times to become progressively less, and for the image-times to grow constant; but in the odd-numbered series 3, 5, 7, 9, 11, the meaning-times are larger and the image-times are smaller than in the even numbered series 4, 6, 8, 10 and 12. The reason is that four words, which were recalled with especial difficulty, happen to fall under the same instruction. For example, in the fourth series these words increased both the reaction-time and the m. v. for image, and in the fifth they had a similar effect on meaning; in the sixth series two of them failed to call up images, and in the seventh two had no meaning. These effects cancelled each other in the total result, but they also tended to obliterate any tendencies of the kind which we observe in R.

We turn next to the statements of our observers concerning the mental processes present when the meanings came. These results we give numerically in Table III.

We are fortunate in having so large a number of cases; the reports came and were recorded as a sort of by-product, and in so far are evidence of the skill of the observers. The record shows unmistakably that for R, at the outset, meanings came largely with visual context, and in the later series, were accompanied more and more frequently by verbal and kinaesthetic processes; for D, on the other hand, meanings almost invariably came visually. If now we compare the total reaction times of Table II with the reports of Table III, we note that R in the second and fourth series and D in the tenth and twelfth series gave meaning-times less than the image-times; yet, of the total of 57 meanings (both observers), 48 came with visual imagery. What is the explanation?

III. *The Method and Results of Moore's Experiment.* Before we attempt an explanation, we may compare these results with those obtained by Moore in an experiment undertaken as a test of the context-theory of meaning.[7] Although, as we shall later see, Moore mis-

[7] T. V. Moore, The Temporal Relations of Meaning and Imagery, *Psychological Review,* XXII, 1915, pp. 177-225.

TABLE III

Observer R

Series	Meanings	Visual	Verbal	Kinaes.	No report
1	14	..	..	..	14
2	14	11	2	1	..
3	14	..	..	..	14
4	13	10	..	2	1
5	15	7	2	..	6
6	14	7	3	1	3
7	15	3	2	3	7
8	14	3	5	4	2
9	15	7	1	3	4
10	14	4	5	3	2
11	15	4	2	7	2
Totals	157	56	22	24	55

Observer D

Series	Meanings	Visual	Verbal	Kinaes.	No report
1	12	12	..	..	..
2	14	6	..	..	8
3	13	11	..	..	2
4	13	2	..	..	11
5	13	11	..	1	*
6	14	13	..	..	1
7	12	10	..	..	2
8	15	10	..	..	5
9	14	11	..	..	3
10	15	14	..	..	1
11	14	12	..	..	2
12	15	13	..	..	2
Totals	164	125	..	1	37

* 1 in word itself.

understood the theory in one important regard, and although he ignored the difference in attitude demanded by the theory, nevertheless he worked by a method of reaction; and the community of method allows us in a certain measure to check his results by our own, and thus to bring them into relation to the theory which he attacks. We proceed, therefore, to give a brief statement of Moore's method and results.

Moore used as stimuli the printed names of familiar objects such as tree, shears, key, fork, *etc.*, and outline drawings of such familiar objects. These he presented one at a time to the observer, who in a typical experiment was instructed as follows: "Ich bitte Sie zu reagieren wenn Sie das Wort verstanden oder seine Bedeutung erfasst, bzw. wenn Sie eine Gesichtsvorstellung von dem durch das Wort bezeichneten Gegenstand gehabt haben."[8] There were three groups of experiments. In the first words were shown, and reactions were made

[8] *Op. cit.*, p. 189.

separately to 'simple meanings' and to visual imagery. In the second words were shown, and reactions were made to meanings-of-purpose and to kinaesthetic imagery. In the third pictures were shown, and reactions were made to 'simple meanings' and to names. "If there is no difference between meaning and the visual image of an object represented by a word the average [reaction-times] of the two [paired] series should be approximately the same (within the limits of the experimental error) . . . . If meaning, however, is produced by or is identical with the visual image which accrues to the sensations involved in the perception of the word, the image series should be shorter if anything than the meaning series."[9] Moore's gross results showed, on the contrary, that the average reaction-times to meaning were almost invariably shorter than those to sensory processes; and he therefore decides against the context-theory of meaning. We are not concerned, however, at this point with Moore's conclusion; his results are here important simply as indicating a certain temporal relation between meaning and content-process.

Moore's observers, then, gave total reaction-times to meaning that were almost invariably shorter than those to imagery. Our observer, R, in the same way gives reaction times to meaning that are shorter than those to image; whereas D gives reaction-times to meaning that are longer than those to image. The temporal relation of meaning to content-process appears, in the light of Moore's result, to be relatively simple; in the light of our own it seems complex. The difference between the two sets of results, if it were uniform, might perhaps be explained by the fact that Moore failed to employ the unfamiliar stimuli which are demanded by the context-theory for psychological contexts, and by the further fact that he failed to inquire whether the logical meanings and content-processes to which his observers reacted were, in fact, correlates. But it is not uniform. What, then, we must again ask, is the explanation?

IV. *The Attitudes set up in our Observers by the Two Instructions.* The explanation is to be found in the conditions of the experiment itself, and particularly in the different attitudes set up in the observer by the instructions. We have seen that Moore ignored this difference; whereas it was our purpose from the beginning to ask our observers, while under experimental conditions, to characterize the two attitudes. Hence, in the series with report we gave the following instruction: "You will be given a stimulus and an instruction to

[9] *Op. cit.*, pp. 189f. We confess we are not clear as to Moore's use of the term 'identical'; if it is taken literally neither of the two possibilities has any bearing upon the context-theory.

react as in the regular series. After the reaction, however, we shall ask you to characterise as best you can the attitude under which you react. We do not want a psychological description of the mental processes which are present; we want rather a statement of what it is you are doing." The task is not easy, because the attitude is set up by the mere acceptance of the instruction; and the obvious reply, when one is asked what one is doing, is to restate the instruction.[10] After repeated trials, however, we obtained fairly satisfactory results. The following are examples of the reports made.

D (Meaning). "When I saw the word I read it over a couple of times. A feeling of familiarity about it. I did not get the meaning until I got an image, a little curly-cue, not the complete picture, one curl of which seemed to be tacked on to the end of the stimulus word."

D (Image). "I read the word once, then an image of a spear-like thing shot out over the stimulus and blotted it out. Under this instruction I am looking for the image which the word calls up; under the meaning instruction I am looking for something which stands for the word, which fills it out. This something is usually an image . . . but the word and image go together."

D (Meaning). 'The instruction for meaning means for me something like 'Do you know what this means?' or, 'Do you know what this is?' The thing I report is not familiarity, and it is not a feeling that I know what it means; but I actually get or grasp the specific, concrete, particular meaning of the word . . . . When I am set for image I am set for the object rather than the pair word-and-object, as is the case under the meaning-instruction; in the former the word may drop out entirely; in the latter I must have both word and object, the word is important."

D (Image). "There seems to be less strain to the image instruction, it seems to have a more definite meaning, I know what an image is when I get it. I have a predetermination that I ought to get the meaning sooner (when under the meaning instruction), and I am on greater strain to report the meaning the instant it comes. I avoid the attitude that I shall get the meaning when I get the image, because I think that would not be following instructions. In spite of the attitude to find meaning as quickly as possible, the image is always there; the difference between the two instructions is as I have reported before, word and image must, for some reason, be together."

R (Meaning). "Under this instruction the intention is not set toward any particular sense department; it is indefinite; set for the cognitive value of the impression rather than the image. I hardly

[10] E. B. Titchener, Description *vs.* Statement of Meaning, *American Journal of Psychology,* XXIII., 1912, pp. 175.

know whether I am justified in saying that 'attention' is set for the cognitive value; the whole disposition is toward it."

R (Image). "While waiting for the stimulus, expectancy that image will come. I feel that I must not react until the image comes. With the meaning instruction I am set to react as soon as it means at all; in case of the image, I wait for the image."

R (Meaning). "The characteristic of this instruction is that I am prepared to react as soon as I can. In the case of the image-instruction I feel that I must wait for the image. In this [the meaning] instruction I wait for the knowledge of what it means. I sometimes know the meaning before the image comes. This happened today when the stimulus was *peruke;* the verbal 'wig' came before the image."

R (Image). "In this instruction I do not care whether I know what the word is, but whether I get an image of it; in the other instruction I do not care whether I get an image so long as I get the meaning . . . . The report of the image is easier than the report of meaning because it is more definite, it is easier to put my finger on."

These reports show that in the view of the observers there is a difference in the attitudes under the two instructions. The characterizations are general, however, and in our attempt to make them more specific we shall avail ourselves of other reports volunteered in the regular series or obtained in the regular report-series. We begin with the reactions to image.

1. *Process-attitudes.* In the early experiments the imagery was, as a rule, not complete at the outset. R says: "Image kept on growing after the report"; "Spot of black changed to assume form of the object"; and D likewise reports: "Part of the imagery came first and then filled out"; "Left hand corner of the picture came first." For R, again, the images throughout continued to be 'hazy', 'not full of detail', 'scrappy'; they rarely reached full development, only once in later experiments did he report a 'detailed representation of the picture'; for D, on the other hand, the imagery was much more complete, it had a good deal of detail in parts and vague outlines in others; as the experiments progressed, the images became habitual, they seemed to appear 'all at once', and they did not change in detail. For D also some salient feature of the object, the feet of *hippogriff,* the spigot of *aquaemanale,* the muzzle of *argali,* came to stand for the object as a whole; the whole, nevertheless, was always represented, even if vaguely. As regards the reactions of the two observers, R was 'set to *wait*' for the image, and reacted at its onset. He was not set to assure himself that the image was of the correct object; it is true he often knew that his image was correct, since verbal and kinaesthetic meanings frequently came before the image; but he says specifically, the "intent is to notice it [the image] when it comes, as opposed to aware-

ness of meaning", and again, "I do not report when the image means but when it comes; . . . the image is just the image, it is not reported as the image of something; this comes later." And there were a few instances in which he announced after the reaction that he had reacted to the wrong image. D, on the contrary, was set to report the image as image of something; the image always referred to some object, it 'was' that object. He says: "I think both word and image stand for an object; the image is image-of, the word is name-of the object. By this I mean that the image is not that of the picture, although it is like it; but the image is of the object, as if I had seen the object." This observer, therefore, did not react until the imagery had advanced further in its temporal course; he usually assured himself that the image 'was correct'; and he frequently rejected the first image that appeared because it was not correct.

It is obvious that the two attitudes to react to image are not the same: R was set by the instruction to report *an* image, regardless of the meaning of that image; D was set to react to a *particular* meaning, which was, however, conditioned upon an imaginal process.

2. *Meaning-attitudes.* We pass to the meaning-attitudes. Both observers frequently stated that the reaction to meaning was the more difficult, because the determination was less definite; there was, as it were, nothing to which one could attend. Their reports show also that there were many meanings, not a single meaning, to which reaction might be made; the instruction, which required a reaction to 'the meaning,' proved therefore to be ambiguous, and as a result the observers did not react to the same thing. We begin with the reactions of D.

At the conclusion of the first preliminary experiment, D says: "Image and meaning now seem to come together. In *peruke, mace,* and *cresset* meaning seemed to come before image but in these cases the meaning was a 'feel', *e.g.,* when the stimulus *mace* appeared I 'felt' that I could describe it, that I could call up the image, and this 'feel' was the meaning. Other stimuli, again, were familiar; I felt that I had seen them before. This was *a* meaning, but I could not call up *the* meaning." D, then, rejected the two 'felt' meanings, and reacted to a meaning which in a later experiment he thus explains: "I am looking for something which stands for the word, which fills it out; this something is usually an image," and then, still later, he continues; "The image adds itself to the word, and the meaning seems to include the word." Thereafter, the meaning was for him specific; other meanings frequently came (*e.g., hippogriff* "meant animal", *costrel* "meant that was the one that troubled me yesterday"; *corbel* meant "it was familiar"), but all these were rejected for what had become to him 'the meaning.' Finally, in the 9th experiment, he says: "If I were to

define meaning now I should say that it is the connection between the word and the image. Neither the word nor the image means in itself; they must be connected, one seems to 'point to' the other, to be directed toward the other. I do not mean that I am conscious of the pointing relation; it is only my way of expressing the thing. The two, word and image, are always together; when I try to hold the meaning I find my eyes shifting, as it were, from one to the other." This observer's predisposition and method are now apparent. He fixated the stimulus-word; the image came and was localised on the exposure-card beside the word; when the word meant the object and the object meant the word, or when the meaning seemed to include both the word and the image of the object, he reacted.

For R, on the other hand, meaning was "knowledge about"; he was set for the "cognitive value of the impression." He found it difficult to be more explicit unless he turned to process. Thus, to the stimulus *sabbaton* he reported, "Hazy image of shoe, and this came in the sense of being the meaning of the word." When pressed for an explanation of 'in the sense of', he added: "Organics in the chest which seemed to mean 'this is the proper meaning.'" In another instance he said: "My intent is to react as soon as I am aware of meaning regardless of process. I react to knowledge"; and when asked what it is to be aware of meaning, he replied: "Possibly a kinaesthetic feel that I do know; it is something like the feel of familiarity. In this particular case the word *meant*, however. I think the feel of knowing came with the meaning." He reacted, therefore, to the first bit of knowledge about the stimulus that came, and he did not trouble himself about the nature of his meanings. His reports show, nevertheless, that the range of his meanings was circumscribed; he did not, *e.g.*, react as a rule to familiarity; and when (on two occasions) he did so he reported: "I reacted to familiarity, not to meaning." The range of meanings to which he did react can best be shown by examples. In the preliminaries every meaning came with visual imagery, and here the image was the object in exactly the same sense as in D's reaction under the image-instruction; "argali", R says, "is that image." But early in the regular series there began to appear kinaesthetic meanings. Thus, *burin* "came with kinaesthetic image of hand making a cutting motion," *mace* "kinaesthesis in arm as if hacking," and Verbal meanings also appeared: *mino* "coat"; *koto* "Japanese"; *hippogriff* "animal". These kinaesthetic and verbal meanings, as we have already seen, became more and more frequent, and the visual imagery more and more schematic, as the experiments progressed, until in the last three experiments there were instances in which the observer was unable to report at all how the meanings came. We do not forget that the observer reacted to meanings regardless of process, and our attempt to indicate the range of his meanings by reference to process is only a matter of convenience.

We may now summarise the reports of attitude given by the two observers under the meaning-instruction. Both reacted to meaning. But D reacted throughout to a specific meaning, namely the relation* between the stimulus-word and an imaged object, and R reacted to a number of different meanings. Furthermore, both observers reacted to knowledge. But D reacted to his own awareness of the meaning of the stimulus, R to the cognitive value of the stimulus.

We go back to the quantitative results. It is obvious that, since our observers were reacting to different things, they could not be expected to give similar total reaction-times. D under both instructions reacted to meaning; his total times under the meaning-instruction were longer than those under the image-instruction because the task he set for himself in the interpretation of the former, (that of relating the stimulus word to the imaged object) took more time than a reaction to the imaged object itself. R according to instructions reacted both to image and to meaning; his total times under the image-instruction were longer than those under the meaning-instruction because under the former he was 'set to wait for the image,' whereas under the latter he was 'set to react as quickly as possible,' 'as soon as the stimulus meant at all.' The visual image was not in his case so habitual a process as were verbal and kinaesthetic images, a fact shown both by his waiting attitude under the image-instruction and by his early substitution of verbal and kinaesthetic meanings for the visual meaning. The quantitative data also point to this conclusion: we have seen that the image-times tended to become constant, whereas the meaning-times tended to become progressively smaller. Furthermore in the 9th experiment, which occurred after an interval of three weeks, there was an increase in the total meaning-times of more than 100σ, and there was also an increase in the number of meanings that came with visual imagery.

V. *The Attitudes in Moore's Experiment.* What, now, can we say of the attitudes in Moore's experiment? In the interpretation of his results Moore not only ignored (as we have seen) the difference in attitude demanded by the con-

* This type of meaning is discussed and explained below, p. 200.

text-theory, but even when the possibility of a difference was subsequently pointed out to him, declined to entertain it.[11] Fortunately we have his instructions and reports of his observers under the instructions; on this basis we may attempt an analysis of the general procedure of his observers.

The significant passage in the image-instruction reads: "Ich bitte Sie zu reagieren . . . wenn Sie eine Gesichtsvorstellung von dem durch das Wort bezeichneten Gegenstand gehabt haben."[12] And Moore says: "The subject reacted to the awareness of the visual image of the object." The question is, of course: Did the observers react to the visual image *as image?* Or did they react to the image as the *object* designated by the stimulus word? The significance of the reaction-times depends, at least in part, upon the answer to this question. If the observers reacted to the image as image, then they reacted to psychological process; if they reacted to the image as object, then they reacted to a particular meaning of the stimulus-word. They seem, without any doubt, to have reacted to the image as object. For (1) the instruction favors the setting up of the meaning-attitude rather than the image-attitude. The image-attitude has been clearly characterised by our observer R: "The intent is to notice it [the image] when it comes, as opposed to awareness of meaning . . . I do not report when the image means but when it comes; . . . the image is just image, it is not reported as the image of something; this comes later." Moore's instruction, however, calls for the *image of the object*. (2) Moore further weights the tendency to react to the object by asking his observers to characterise their experience after the reaction, "und dabei anzugeben, ob die aufgetauchte Vorstellung an die Stelle der Bedeutung gesetzt werden konnte, etwa bloss die konkrete anschauliche Erfüllung dessen war, was in der Bedeutung abstrackt intendiert wurde."[13] We question the possibility of following this instruction unless the image is regarded as object; the observers were set for a meaningful experience, and they were expected to compare meanings. (3) In the few reports which Moore quotes the images are given as objects and not as processes. "It [the image] looks like a large rocking chair"; "I imagined a part of a rake"; "Then there appears the color of the animal, I see 'brown'"; "The image did not

[11] *Op. cit.*, p. 212.

[12] *Op. cit.*, p. 189.

[13] *Op. cit.*, p. 189. The fact that Moore required this report of his observers indicates a misunderstanding of the context theory. In a later connection (p. 218) he says also: "It remains for Professor Titchener to prove that meaning is *identical* [italics mine] with the concomitant or subsequent imagery." On the contrary, the context theory has never stated that the content-process to which it appeals is even a *representation* of the logical meaning.

really represent a radish but a turnip," "Then arose the head of an ox with his horns as drawn in the pictures of these experiments."[14] A report of process, on the other hand, would have described the image as image, *e.g.* as hazy, scrappy, colorless, or the contrary, and as running a certain temporal course. Our observer R, it will be remembered, reacted at the onset of the image; he was not set to know whether the image was of the correct object; and after the reaction he occasionally declared that he had reacted to the 'wrong' image.

All these considerations point to the conclusion that Moore did not realize the difference between the image-of-object and the image as image; he did not see that the first is a particular meaning of the stimulus-word, whereas the latter designates a psychological process. If he had instructed his observers to react to a visual image, to the first visual image which came up, and had trusted to the situation to bring the correct psychological image, then he might have obtained reactions to content-process. But our own results have shown that the tendency toward 'objective reference' is so great, even in the case of experienced observers, that the instruction needs to be more explicit.

The same criticism holds, in principle, of Moore's other experiments. In the third, where he exposed pictures of familiar objects and asked his observers to react with the name, the chief concern was, apparently, to find a name which meant the stimulus, and not to react to a verbal-process. Only two reports from the word series are published;[15] in the one the observer says: "I knew that the meaning of the word was more general than that of the picture"; in the other, one word-meaning (*Tulpe*) was rejected for another (*Glockenblume*).

Finally, in the second experiment the observers were shown words with the following instruction: "Ich bitte Sie zu reagieren wenn Sie die Bedeutung des Wortes im Hinblick auf den Gebrauch oder die Funktion des damit bezeichneten Gegenstandes erfasst, bzw., wenn Sie eine kinaesthetische oder kinaesthetisch-optische Vorstellung davon gehabt haben."[16] According to the seven reports of imagery which Moore quotes, the procedure of the observers was first to image the object visually, and then to represent the purpose of the object in visual-kinaesthetic imagery. In five of the seven cases the observers

[14] *Op. cit.*, p. 197ff.

[15] *Op. cit.*, p. 211. Moore gives the second of these reports in the 'meaning' column; but reference to the table of reaction-times (Lehner, 209) shows that on the date indicated the reaction was, in fact, to the 'word.'

[16] *Op. cit.*, p. 201.

themselves report that the imagery did not completely fulfil the meaning of purpose. For example: *Lampe* "I imaged the lamp that I use in my dwelling, and saw clearly that it did not burn brightly enough, and then imaged the turning up of the wick. The kinaesthetic image of the movement cannot be identified with the consciousness of purpose."[17] *Trichter* "Immediately after the simple meaning of the word, I had the visual image of a funnel and then the kinaesthetic image of laying hold of it with my right hand and placing it over an opening. Here also the kinaesthetic image falls short of being the fulfilment of the purpose. For I think that the funnel is the instrument by means of which I pour fluid through an opening, and my image is only the placing of the funnel in the opening."[18] The obvious inference from these cases is that the observers reacted to the visual-kinaesthetic-image, not as image, but as meaning.

In all three experiments processes,—visual imagery, verbal-kinaesthesis, and visual-kinaesthesis,—were present; that we freely admit. Again, under all three instructions it is possible that the observers might have reacted to processes and not to particular meanings; that we also admit. But we have shown that Moore has failed to guarantee reactions to processes, that his instructions would tend to set up attitudes for meanings rather than for processes, and that the reports of the observers themselves indicate that they reacted, in fact, to meanings instead of processes.

VI. *Various Types of Meaning to which our Observers Reacted.* In the discussion of the attitudes of our own observers, we saw that they responded to the experimental situation with several types of meaning. When the experimental series of D and R were concluded, we thought it advisable to make a further study of these various meanings with a new observer. Accordingly we gave the same instruction that we had used with D and R to Dr. W. S. Foster (F), then instructor in psychology, who had had previous experience in meaning-experiments.[19] F demanded a further explication of the term 'the meaning' which occurred in the meaning-instruction. The only answer that seemed legitimate was to instruct him

[17] *Op. cit.*, p. 207.

[18] *Op. cit.*, p. 207.

[19] E. Jacobson, On Meaning and Understanding, *American Journal of Psychology*, XXII, 1911, p. 553; Titchener, Description *vs.* Statement of Meaning, *Ibid*, XXIII, 1912, pp. 174ff.

further to react to the first meaning of which he was aware. The result was that he reacted to a number of types of meaning, most of which had already been reported in the earlier series by our other observers. We then changed the instruction so as to obtain reactions to particular types of meaning. The details of this new instruction, together with the quantitative results which we regard as indicative of the temporal relations of these meanings, we shall present in due course; for the present we propose to discuss in some detail the meanings themselves, and in so doing we shall avail ourselves of the statements of all three observers.

By way of preface three things should be said. (a) The list of meanings is purely provisional; it is the result of this experiment only, and future work will doubtless show the need of restatement. (b) We have tried to limit ourselves absolutely to the meanings of which the observer was aware at the moment of reaction. The observer may, in the explication of a meaning, state a new meaning, that was not present at the moment in which the reaction occurred.[20] The experimenter must always be on guard against such intruders. (c) The order in which we discuss the meanings is insignificant.

(1) The stimulus-object is *familiar*. The familiarity was verbally explicated by some such phrase as, "I have seen it before," "I had that word yesterday."[21] Occasionally the meaning is more specific, *e. g.*, "That is the word that gave me trouble yesterday." This meaning was consciously rejected by D on the ground that it was not 'the meaning;' it was not considered as a possible meaning by R; and it was frankly accepted as 'the meaning' by F. Psychologically, the context was a diffuse sense-feeling, the 'feeling of familiarity,' a 'glow.'

(2) The stimulus-object is *known*. The observer is aware that the stimulus has a meaning which he could work out in definite form. F's usual statement was: "I'm all right to get the meaning." This meaning was, again, rejected by

[20] *Cf.* G. E. Müller, *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, I. 1911, pp. 65f., 137ff.; E. B. Titchener, The Schema of Introspection, *American Journal of Psychology*, XXIII, 1912, p. 503.

[21] This type of meaning is reported also by A. Binet, *L'ètude expérimentale de l'intelligence*, 1903, p. 74.

D; it was probably the basis of some of R's reactions; and it occurred frequently in the case of F.[22] Psychologically it was said to be carried by a "feeling that I know," "a kinaesthetic feel, something like familiarity," a feeling of "relaxation" localised principally in the eye-muscles. F reported cases in which the feeling of familiarity or the 'glow' fused with this relaxation, and he also reported meanings that were partly familiarity and partly "I'm all right."

(3) The stimulus-object is *something which arouses this sort of feel.* The nature of these meanings, which were reported frequently by F and occasionally by R, can best be shown by examples. F says: *poulaine* "I reacted to the attitude I have for men wearing tights and pointed slippers;" *quern* "Means attitude for something Irish and old;" *pschent* "I reacted to the conscious attitude that stands for ancient Egypt;" *brob* "Means the feel I get for a blunted-at-each-end thing, a feeling which, if explicated, is horizontal and blunted at both ends; the real brob is wedge-like;" *kylix* "A kinaesthetic set which means it is that kind of a thing." R similarly states: *dagon* "A kinaesthetic feel as if back were arched."[23] Psychologically, these meanings correspond with conscious attitudes which for the most part remain unanalysed; in the last two cases mentioned above, a kinaesthetic component was clear enough to be reported. Finally, the meanings of this type may precede, may follow, and may accompany the form of meaning given under (2).

(4) The stimulus-object is *this kind of movement.* It is a movement which, regarded as an expressive movement or gesture, is *as if* something were predicated of the object. For example, *mace* meant a movement as if to strike; *burin* as if to cut; *jougs* as if to encircle. For F such a meaning occa-

[22] Meanings of this type are also reported by H. J. Watt, Experimentelle Beiträge zu einer Theorie des Denkens, *Archiv für die gesamte Psychologie,* IV, 1905, pp. 317ff; A. Messer, Experimentell-psychologische Untersuchungen über das Denken, *ib.,* VII, 1906, pp. 71, 77; J. R. Angell, Thought and Imagery, *Philosophical Review,* VI. 1897, 648. They are described by G. F. Stout in his summary of 'implicit apprehension,' in *Analytic Psychology,* 1909, I, 95.

[23] Messer, *op. cit.,* pp. 77-80, includes this type under the term *Sphärenbewusstsein.*

sionally followed a meaning of the preceding type and rendered it more definite; *e. g., skean* was at first a feel of a long lean thing, then a movement as if striking something. It is important to note that the two observers who report these meanings insist that the meaning *is* the movement, or as F put it: "*Besague* is *this*," and he makes a movement as if cutting something. Furthermore, both observers think that the nearest approach to the meaning in spoken language is a verb; *e. g., mace* means strike, *burin* cut, *brob* drive, *etc.;* the two former do not mean objects with which one strikes or cuts, and the latter does not mean an object which one drives (as a nail).

(5) The stimulus-object is *that object.* In these cases the object was always present in visual imagery, but at the moment of reaction the image was as truly the object as if this were given in perception.[24] It is characteristic of all our observers that some salient feature of the object comes, in the later experiments, to carry the meaning. For D the object as a whole was also represented in imagery, even if vaguely, whereas for R and F the salient feature ultimately stood alone. In such cases there was no conscious reference from the salient feature to the object as a whole; and in one instance, after the experiment had come to a close, the observer was unable on reflection to say more about the object than that "It was something that had a spigot." (The stimulus was *aquaemanale.*) There were cases, however, when upon reflection the observer knew well enough that the visual object to which he reacted was not the object which belonged to the stimulus-word; *e. g.,* R said on one occasion, after reacting to the meaning of *aplustre,* "If you were to show me the image I had and ask me to name it I should not say 'aplustre.'"

[24] In the early series when the meaning of the stimulus was as a rule the imaged object, F frequently reacted to the meaning "I'm all right to get the image" (a type that belongs to group 2). In the later series this meaning was well known also to R, who thinks it possible that in the early series he often reacted to it instead of to the imaged object itself. This, we think, is the probable explanation of those series of R (see p. 186) in which the meaning-time was less than the image-time, while yet the meaning was reported as 'image.'

(6) The stimulus-object is *this name*. The meaning of this type most nearly approaches the definitional meaning of logic. The general form is a definition of the stimulus in intension; *e. g., argali* goat; *peruke* wig; *mino* coat; *hippogriff* animal, *etc.* In some cases, however, the meaning is in extension; *koto,* for example, is a Japanese musical instrument, and R reacted to this stimulus with the word 'Japanese.'[25] We believe that usually if not always the name was first applied to the object when pictured or imaged. In our attempt to force a visual learning of our stimuli, we required our observers to inhibit a verbal response; yet in the first experiment R reacted to *peruke* with 'wig.' In the later experiments R reacted to *pavon* with the word 'spear;' *pavon,* however, is not a spear, but a pennon fastened to the shaft of a mediaeval lance; the lance became the salient feature, and was visualised without the pennon, as the object; finally, when the object dropped away and the word took its place, 'spear' was the meaning of *pavon.*

(7) To these six we may, from the reports of D, add as a last type of meaning: The stimulus-word is *related to the stimulus-object.* It will be recalled that D reacted when the stimulus-word and the imaged object were in the same visual field. His report runs: "It [meaning] is the connection between the word and the image. Neither the word nor the image means in itself; they must be connected; one seems to 'point to' the other, to be directed toward the other. I do not mean that I am conscious of the pointing relation; it is only my way of expressing the thing." D later remarked that, when he accepted the instructions, he interpreted the term 'meaning' in the sense of something that the stimulus-word 'stands for' or 'points to.' In the earlier experiments this 'something' was always the imaged object, and so, the spatial contiguity of stimulus-word and imaged-object came eventually to touch off the meaning-response.

Here, then, are seven 'types' of meaning, logically distinguishable. No doubt there are still other types which further experiments might have revealed; no doubt also other

[25] For similar meanings of this type see Messer, *op. cit.,* p. 81.

interpretations of a general instruction to 'react to meaning' may be made under other experimental conditions.[26]

VII. *The Types of Meaning to which Moore's Observers Reacted.* It will be instructive at this point again to examine Moore's paper with a view to the interpretation which his observers put upon the meaning-instruction, and the nature of the meanings to which they reacted.

As we have already seen, Moore performed three groups of experiments; the first and third had an instruction which was practically the same as our own; his stimuli, however, were familiar words, whereas ours were unfamiliar. The significant phrase of Moore's instruction reads: "Ich bitte Sie zu reagieren wenn Sie das Wort verstanden oder seine Bedeutung erfasst haben."[27] The obvious sense of this instruction is that the observers were to react when they understood the word or grasped its meaning. But Moore himself says: "In this series, therefore, the subject reacted . . . . to the awareness that the word had a meaning." Now it is one thing to understand a word, or to grasp its meaning, and quite another thing to be aware that the word has a meaning. What, then, did Moore's observers really do? We are not told; but we find that, of the fifteen statements of meaning which Moore quotes, only four (two by Moore, one by Külpe, and one by Marezoll) conform to Moore's own interpretation of his instruction. These meanings fall under our own second type. For example, Marezoll says: "I knew that the word was something with which I was familiar, and knew that from this point I could get the meaning."[28] Moore makes a similar statement: "At first a feeling of familiarity was present and then a feeling of certainty that I knew what the word [shears] signifies without analysing its meaning further. First during the reaction itself there came the further thought 'something with which one cuts'."[29] Külpe likewise reports: "Immediately after I saw the picture [of a horse] I experienced a tone of familiarity and knew what this picture represented . . . . The tone of familiarity was related not to the picture but to what it signified. The picture was a symbol of real objects and its signification consisted herein, *viz.*, to point to them."[30]

Of the eleven remaining statements of meaning quoted by Moore, six

[26] Both Binet, *op. cit.*, pp. 73f, and Messer, *op. cit.*, pp. 73ff, 75(4), report cases in which the stimulus-word had no meaning other than as bare word; and Messer in this connection also reports the meaning of strangeness.

[27] *Op. cit.*, p. 189.

[28] *Op. cit.*, pp. 197, 208. We refer to the first of the two meanings here reported.

[29] *Op. cit.*, p. 197.

[30] *Op. cit.*, p. 211.

give meanings-of-purpose, four imply general concepts, and one apparently corresponds with our sixth type, *i. e.*, is a name.[31] All but one of the meanings-of-purpose were obtained under a special determination to react to meanings of this kind; the exception is a reply of Külpe's to *Dampfschiff*: "A means of transport by water." [32] Two of the others were purposive concepts of a class of objects and were also cast in verbal form: *e.g., eye-glasses* means "The correction of an error of refraction," and *clock* "Tells the time of day;" the rest were of particular objects, and in every case the particular object was also visualized: *e.g., chain* "I pictured to myself a tolerably strong chain and remembered . . . . that such chains are used to tie animals in their stalls;" *fork* (Gabel) "An instrument for eating, accompanied by a weak visual image of a fork." It might be mentioned in passing that if the stimulus, in these cases, had brought up a different kind of visual object, then the meaning of purpose would have been different; *Gabel,* for instance, might have brought visual imagery of the frog of a horse's foot, which has a purpose different from that of table forks.

One of Moore's meanings which we have classed as a general concept may be a visual object. To the stimulus *Geier,* Marezoll gives the meaning: "I *knew* that it was something that hovers over mountains in the air—even though I did not see the mountains. Visually I imaged only a pair of extended wings and *knew* that something belonged between them." [33] The others are undoubtedly general concepts; all are given by Külpe.[34] Examples are: *Kerze* "There came to me at once the word 'light.' This was not a determination of the meaning, but only another word for it. The meaning was entirely general, as if I should say a candle, that is, any candle—every possible candle;" *Veilchen* "Immediately after the word appeared I had an auditory-kinaesthetic image of it as I pronounced it, *Veilchen,* and in connection therewith a general knowledge of its meaning that I can thus explain: a definite species of flower. I dare say that it is this which makes up the content of the meaning—what I actually knew about this object during the experiment.

The net result of our examination of that part of Moore's paper which deals with meanings is (1) that his observers interpreted differently the meaning-instruction, and (2) that the only types of meaning which differed from those found in our experiment are meanings-of-purpose and general concepts. It is worthy of note that all of the latter kind were

[31] *Op. cit.,* (Lehner) p. 211; see footnote 15 of this paper, p. 195.

[32] *Op. cit.,* (Külpe) p. 198; (Lehner) pp. 206, 207; (Külpe) pp. 206, 207; (Moore; second meaning) p. 208.

[33] *Op. cit.,* p. 198.

[34] *Op. cit.,* pp. 197 (two meanings), 198.

furnished, under the general instruction to react to 'the meaning,' by Külpe, who was epistemologist as well as psychologist; and that all but one of the meanings-of-purpose were obtained under the express instruction to react to such meanings.

VIII. *The Temporal Relation of Different Types of Meaning.* We now return to the quantitative results obtained from F. Before we began these experiments we had noticed that the reactions of Moore's *O*'s to meanings-of-purpose were twice as long as those to simple meanings, and that D's reactions to 'imaged objects' were almost invariably shorter than those to his 'relational meanings;' we had also found that the average time of Names reported by R in the last four series was 499 ± 109, whereas that of Objects was 572 ± 119 (the probable correctness of the difference of these two averages is 93%). Since F in the preliminary series had reacted to meanings of various kinds we hoped through him to get some knowledge of their quantitative relations. It will be recalled that F began with the same instruction as D and R. In Table IV we give the separate times of all the meanings of the three series in which F reacted under this instruction, *i. e.*, to the first meaning of which he was aware, and also (in the last column) the averages of his times under the 'image-instruction.'

The Table shows, first of all, clean-cut *differences of average within the series.* We have averaged only those times, under a given heading, of which there were at least three cases; and in no instance is the probable correctness of the difference between averages less than 99%. The 'image'-times, though obtained under the image-instruction, represent reactions to the imaged object (not to the psychological image). They should, therefore, be approximately the same as the average Object-times obtained under the meaning-instruction. That this is the case is shown, at the foot of the table, by the probable correctnesses of the differences.

A second noteworthy feature of the table is the *decrease of average in successive series.* The times of Objects (and 'images') decrease in magnitude progressively throughout the experiment. Other meanings, whose times at all stages

TABLE IV

REACTION TIMES OF F TO VARIOUS TYPES OF MEANING IN SERIES 1, 2, AND 3 (GENERAL INSTRUCTION)

Series	Fam.	All—right	Feel.	Obj't.	Name	Undetermined	Image
1		595 624 652 624±19		2849 1106 1036 1202 1035 1029 1467 1403 ±460	1677	746 798 1160 707	Average of 13 cases 1342±707
2	(515)	465 578 505 513 515±31	655 706 565 639 641±37	1376 1773 735 1293 ±374	756		Average of 14 cases 1024±518
3	(566) (502) 677 656	698 529 663 663 629±68	608 604	880 668 991 846±119 (2668) (2658)			Average of 14 cases 784±138

Series 1—Av. Obj't-Av. Image (1403-1342) 61. Prob. Cor. of Difference 57%.
Series 2—Av. Obj't-Av. Image (1293-1024) 269. Prob. Cor. of Difference 76.5%.
Series 3—Av. Obj't-Av. Image (846-784) 62. Prob. Cor. of Difference 75%.
The cases in parentheses are half Familiarity and half All-right. Those in brackets are delayed meanings.[35]

are less than those of Objects, increase in number in the later series. As the work proceeds, under the general instruction, Object gives place to Feel, All-right, and Familiarity; meanings of a concrete and imaginal kind to those of a vaguer and 'attitudinal' sort. Both results may presumably be attributed

[35] No adequate meaning came at once and the observer sought the visual image. In the one of these a Familiarity and in the other a Feel came just before the reaction to the Object.

to practice; but practice works in the one case by simple decrease of time within a given category, and in the other case by change of recourse from the more to the less time-taking mode of reaction.

IX. *The Temporal Relation of Meaning and Correlated Process.* In discussing the types of meaning (pp. 196ff) we made mention of their correlated processes. For example, F reported a sense-feeling, a 'glow,' as the correlate of Familiarity, and a 'relaxation,' localised principally in the eye-muscles, as that of All-right. It is obvious that, if the attempt is made to investigate by the reaction-method the temporal relation of meanings and processes, Familiarity must be paired with 'glow,' All-right with 'relaxation,' and so on. We accordingly formulated a series of six instructions, as follows:

(1) You are to react to meaning: If Familarity comes react to it.
(2) You are to react to meaning: If All-right comes react to it.
(3) You are to react to meaning: React when you have grasped the meaning.
(4) You are to react to process: If 'glow' comes, react to it.
(5) You are to react to process: If 'relaxation' comes, react to it.
(6) You are to react to process: If visual image, kinaesthesis, *etc.*, comes, react to it.

The average reaction-times and the probable correctness of their differences, obtained in Series 4-7 (in which the above instruction in whole or in part was employed), are shown in Table V.

Looking first at the meanings, we see that the average times of Familiarity in Series 4 are shorter than All-right; the probable correctness of the difference is 94.5%. The average times of All-right in Series 6, however, show that these meanings may with practice and under a single determination become as ready as Familiarity. The average reaction-times of the correlated processes tell a similar story. The average time of 'glow' in Series 4 is less than that of 'relaxation;' the probable correctness of the difference is 99.7%. In Series 7, though 'glow' is again less than 'relaxation,' the probable correctness of the difference is only 72%. The times of All-right in Series 4 are shorter than those of 'Meaning is grasped,' whereas 'relaxation' is longer than image, kinaesthesis, *etc.* We return to this variation later.

TABLE V

AVERAGE REACTION-TIMES OF F TO VARIOUS TYPES OF MEANING AND TO CORRELATED CONTENT-PROCESSES IN SERIES 4, 5, 6 AND 7 (SPECIAL INSTRUCTION).

Series	Meaning			Content-processes		
	No. of cases	Type		No. of cases	Type	
4	10	Familiarity	540±37.5	10	"Glow"	559±42
	10	"All-right"	602±94	10	"Relaxation"	684±102
	9	Meaning	726±106	10	Image, kin., etc.,	617±82
5	10	"All-right"	617±42			
6	11	Familiarity	531±78			
	9	"All-right"	534±65			
	5	Fam. All-R.	517±42			
	3	Feel.	531±45			
	1	Unidentified	1001			
7				10	"Glow"	494±54
				10	"Relaxation"	511±59

Probable correctness of the difference (Series 14) between:

Familiarity and All-right	94.5%
M. is grasped and All-right	98.2%
Relaxation and Glow	99.7%
Image, etc. and Relaxation	90.0%
Familiarity and Glow	80.0%
All-right and Relaxation	93.3%
M. is grasped and Image, etc	97.4%

What conclusion, now, may we draw as regards the temporal relation of meaning and process? The reader may think that the number of cases is insufficient to any conclusion. We shall, however, venture upon a discussion.

It will be recalled that Moore assumed the adequacy of the reaction-method to determine the temporal relation of meaning and correlated process. Thus far we have not questioned this assumption; we have been content to set forth the conditions which must be known and brought under control before the two sets of times may properly be compared. We now, however, suggest the possibility that a difference between the averages might be due, not to the temporal relation of meaning and process, but to the time required for

reaction itself. Just as reactions to sound are shorter than those to smell, reactions to tonal intensity shorter than those to quality, reactions to minor shorter than those to major chords, so it might be that reactions to meaning are shorter than those to process. The organism is indubitably more accustomed to react under the meaning instruction or in the meaning attitude. The first two lines of the Table seem, in fact, to bear out this hypothesis; for Familiarity is somewhat shorter than 'glow,' and All-right than 'relaxation.'

There is, however, a second possibility. Meaning-time and process-time may be identical within the limits of variation of the reaction course. In support of this hypothesis, the Table shows, as we have seen, that the temporal relations of Familiarity and All-right are paralleled by those of 'glow' and 'relaxation' (Familiarity in Series 4 is shorter than All-right, 'glow' than 'relaxation;' All-right shows the effect of practice more than Familiarity, and 'relaxation' than 'glow'). Furthermore, the relative m. v.'s in Series 4 are almost exactly the same for Familiarity as for 'glow,' and for All-right as for 'relaxation.' The third line of the Table may also be interpreted in favor of the hypothesis. The meanings were, for the most part, Objects and Names; the correlated processes were visual images and kinaesthesis. We have here, on the one hand, a complex of processes which may touch off a reaction at an early stage of its course, and on the other hand a meaning which can emerge only when the complex has taken on a fairly definite form.[36]

In view of the complexity of the subject any more definite outcome could hardly have been expected. The results seem to warrant a further investigation in which after a certain type of meaning and its process-correlate have emerged, very definite instructions shall be given to the observer and a long series of paired reactions taken. Even so there is, of course, no guarantee that the effect of practice upon the two series will be the same.

X. *Summary.* This investigation was undertaken as the study of a part-problem within the context-theory of meaning. While it has shown that the subject is far more complex

[36] See R's report pp. 190f.

than existing formulations of that theory, or previous experimental work, had indicated, it has, nevertheless, revealed nothing out of accordance with the theory. Special results may be summarised as follows.

(1) The meaning-attitude is that of establishing the signification or *reference* of a stimulus-object. The process-attitude, on the other hand, is that of reporting or describing psychological experience *without reference* beyond itself.

(2) The meanings which emerge are individual or prelogical meanings. Under a general instruction to react to 'the meaning' various types of meaning appear, which are logically distinguishable and which, within the limitations of practice, have different average reaction-times.

(3) The method of reaction thus promises to be useful in this sort of inquiry; with certain experimental safeguards, it appears adequate to a distinction of types of meaning and, combined with statements of meaning and with introspection, it promises to throw light on the temporal relation of meaning and correlated-process.

(4) An incidental analysis of Moore's method and results shows that the conclusion which he has drawn, adversely to the context-theory of meaning, is not substantiated.[37]

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[37] See also an article which has appeared since this paper was written; E. C. Tolman, More Concerning the Temporal Relation of Meaning and Imagery, *Psychological Review,* XXIV, 1917, pp. 114-138.

THE DISTRIBUTION OF TIME IN LEARNING SMALL AMOUNTS OF MATERIAL

By A. S. Edwards, University of Georgia

"Jost's law," writes Meumann,[1] "does not seem to be valid for smaller and easier materials, where we find that uninterrupted learning until memorization is reached proves to be more advantageous. We need scarcely do more than indicate the pedagogical significance of Jost's law. Whenever the child is obliged to master a *voluminous* memorial material he should be given instruction and opportunity to learn it in easy stages; the memorization should not be forced but the repetitions should be distributed as widely as the prescribed course of teaching permits."

The present study was made for the purpose of discovering exactly what differences in reproduction would appear when groups of equal ability studied as small as one typewritten page of material double-spaced, the one with a review period, the other without a review period, the total time given to study being the same for both groups.

Method

Fourteen experiments in history and arithmetic have been completed, including a total of 730 cases. In each experiment the students were divided into two groups, the members of which were equal in scholarship on the basis of the school records. One mimeographed page was laid face downward before each pupil. The pupils were instructed that they were not to look at what was written until the signal was given. They were then to turn over the sheet and study until they were told to stop. They should then turn the paper over again immediately and write all that they could remember on another sheet which had been placed before them before

[1] E. Meumann, *The Psychology of Learning*. Tr. by J. W. Baird. D. Appleton & Co., 1913. Pp. 266-7.

they began to study. No instructions were given as to the method of study. The pupils were told that they were to learn all that they could in the time given and that they would be asked to write all that they could remember.

The non-review groups were permitted to study for 6½ minutes and then given exactly 12 minutes to write all they could recall. The review groups were permitted to study 4 minutes, then given exactly 12 minutes to write, and some days later were given 2½ minutes to review. Still later both groups were given an examination at the same time. After each learning period the pupils were led to believe that that was the end of the test.

The method allowed exactly the same amount of time for all who were tested. The one group had a review, the other had not.

The fourteen experiments include pupils in the sixth, seventh and eighth grades; high and normal schools. Experiment 1 in history was too difficult. Experiments 2-9 were done with an easier page of history. Experiments 10-14 were on the metric system. This test involved arithmetical reasoning although it was a good deal memory work. Care was taken to have tests entirely new to those who took them. All papers were graded according to a scale to insure uniformity.

The intervals between study, review and examination differed on account of the necessity of adapting to the conditions under which the classes could be had. The intervals in days for the review groups in the 14 experiments were respectively: 4 + 2, 7 + 6, 1 + 3, 7 + 7, 7 + 7, 2 + 3, 1 + 4, 5 + 4, 3 + 5, 7 + 7, 7 + 7, 7 + 7, 7 + 7, 7 + 7. For the non-review groups: 6, 13, 4, 7, 7, 3, 4, 4, 5, 7, 7, 7, 7, 7.

The Factor of Recency

As may be seen from these figures, in order to check the factor of recency, the non-review groups in the first 3 experiments had their study when the review groups studied, and in all other experiments had their study when the review groups reviewed. Therefore the review groups had the advantage of greater recency in the experiments 1-3, but not in the others.

RESULTS

Exp.	Non-review Groups NC	TFR	AV.	MED.	M.V.	Review Groups NC	TFR	AV.	MED.	M.V.	Per cent Sup. Rev. Gr. Acc. to Med.
1	12	99	8.2	5.5	6.9	10	159	16.0	15.5	9.7	180
2	43	574	13.3	15.4	7.1	43	1120	26.2	27.6	9.9	79
3	10	309	30.9	33.0	9.5	14	522	37.6	40.0	10.0	21
4	14	237	14.0	15.0	6.6	18	319	17.4	17.5	6.8	17
5	17	236	13.8	13.4	3.3	10	160	16.0	16.7	3.8	24
6	17	260	17.0	16.0	5.3	14	264	19.0	19.5	7.7	22
7	47	1246	26.5	27.0	7.7	36	1322	36.7	37.0	4.4	38
8	51	1365	26.8	26.0	7.6	55	1771	32.2	29.5	9.5	14
9	73	1956	26.0	26.4	7.5	81	3008	37.2	36.6	9.1	39
10	14	103	7.4	7.0	4.2	13	148	11.4	10.5	7.8	50
11	10	119	11.9	13.0	4.7	11	167	15.2	17.5	7.9	35
12	15	111	7.4	2.5	6.9	9	118	13.1	13.5	5.0	440
13	16	164	14.0	10.5	10.2	18	299	16.6	14.0	7.1	30
14	26	372	14.3	12.5	6.0	33	596	18.0	15.9	7.0	27
	365	7151				365	9973				

Explanations: The first column gives the number of the experiment. NC = number of cases. TFR = total number of facts recalled. Med. = median. Av. = average. M. V. = mean variation. The last column indicates the superiority of the review groups in per cent, on the basis of the medians.

The average superiority of the review groups in the first three experiments where the review groups had the advantage of recency is 93 per cent. The average superiority in the other experiments is 67 per cent, and if we eliminate experiment 12, it is just less than 30 per cent. The extreme result in this experiment is due to the fact of several failures and near failures in the non-review group.

The Relation of the Value of Reviews to the Difficulty of Learning

It occurred to the writer to list the experiments in the order of difficulty, taking into account the difficulty of the material and the age of the students, in order to see if there was any relation between the value of the review and the difficulty of learning. If we do this for the first three experiments, where the review groups had the advantage of recency, we find the

figures are respectively 180, 79 and 21, the most difficult being first, the least difficult last. If we throw together the groups in the remaining experiments where the difficulty was, according to the judgment of the experimenter, approximately the same and take the average for each group we have the following: 138 (38, if we eliminate experiment 12), 27 and 22. So far as these experiments go, the review seems to be more valuable for more difficult learning. There is the suggestion of a general law, *i. e.,* that the value of reviews is directly proportional to the difficulty of learning. The generally recognized fact that the distribution of time is of value especially or only for voluminous material would help to substantiate this conclusion. The experiments here reported are too few to more than suggest the possibility of such a law.

The results of experiments with two college classes also substantiate this conclusion as a careful examination of this data shows that we may expect the review groups in college classes to show a superiority of about 5 per cent. The results of experiments with the college classes were not included in our table inasmuch as excessive absence occurred on the day of the examinations due to a sudden calling of class meetings which was unexpected and absolutely independent of any control by the experimenter. The groups had to be entirely checked over and many eliminated in order to make the groups comparable on the basis of college grades. Even after all this was done it did not seem right to include the results with those of the other experiments.

The reversal of groups has had to be postponed partly because of pressure of other work, and very much on account of the lack of feasibility for each class to give six periods to this work. Three periods have already been taken from class work for each class experimented upon.

Conclusions

1. The experiments show without exception superiority of the review groups over the non-review groups.

2. When the review groups had the advantage of recency in review, the median results of the review groups average 93 per cent better than the results of the non-review groups.

When they did not have the advantage of recency, the median results of the review groups average just less than 30 per cent (and including one extreme case, 67 per cent), better than the results of the non-review groups.

3. Contrary to the statement of Meumann, the division of time in learning small amounts of material, *e. g.*, one mimeographed page, double-spaced, seems to be very important.

4. A law is suggested, *i. e.*, that the value of reviews is directly proportional to the difficulty of the learning.

BLINDFOLD CHESS: THE SINGLE GAME

By K. M. Dallenbach, Cornell University

The writer received his first instruction in the game of chess in the fall of 1911. Six weeks later (Nov. 4) he played his first "blindfold" game. He had for many years played checkers (draughts), twice winning the championship of his University; and it is probably owing to his familiarity with this game that he readily took up chess. He attempted the blindfold play in rivalry with one of his playing companions who had been able to carry a game through twenty moves. To his great surprise he successfully carried the game through a pawn-ending to a mate, the play running to sixty-three moves.

During this game he was not blindfolded. He sat with his back to his opponent, and as an aid to the play he looked at a chessboard hung upon the wall about three feet in front of him. The board of play used in almost all his visual playing was of paper, with black and red squares; the board hung upon the wall was of glass, with black and pearl squares. The task involved a considerable mental strain; and recurrent visual images of the board and of the pieces (which latter had been seen only in imagination) persisted for several days.

"Blindfold" play was not attempted again until March 4, 1912, when the second game was played and the present study undertaken. In this, as in all of the succeeding games which form the basis of the present report, the play was made blindfold, in order that the conditions from game to game might be kept as uniform as possible. A few days later (the fifth experience with blindfold chess), two games were played simultaneously; and on the next day, three simultaneous games were successfully played. The mental processes involved in the multiple play proved, however, to be so widely different from those involved in the single game that the former was given up and attention was concentrated upon the latter. It is the writer's intention to return to the subject of multiple play

in a later study; here the psychology of the single game is alone reported.

In all games after the first the writer, whom we shall henceforth refer to as the observer (*O*), was blindfolded and sat with his back to the board of play. He preferred this position, and felt a slight discomfort when his opponent and the board were not so situated. The preference is probably due to the position assumed in his first game. At frequent intervals during the play he jotted down the introspective notes which form the basis of this paper. His opponent, or a third person, kept a record of the moves and of the time of the moves; noted the point in the game and the time at which *O* recorded his introspections; and also noted the changes of conscious attitude and of feeling that *O* reported to him. The English notation was used in designating the moves, which were transmitted orally. Acording to the rules governing handicap play, *O* always had the white pieces and moved first.

It need hardly be said that the analysis of the processes involved in the game can, under these circumstances, be carried only to a certain point. The blindfold player begins with a certain knowledge of chess, so that his 'visual images' and 'kinaesthetic complexes' carry always a reference to the game; he is set chessward from the very first; and the utmost that he can do is to indicate the sort of mental formation by which on the particular occasion this reference to chess is carried or in which it is embodied. Here, then, is a fixed limit to the progress of analysis, given with the very nature of the problem; *O* cannot go behind his point of departure. There is also a variable limit, set by the complexity of the processes to be observed. *O* is under a twofold determination: he sits down to play a game, and if possible to win it; he sits down also under the instruction to interrupt the game, at its psychological turning-points, and to give a psychological report of his experience. The shift of attitude which this dual task implies becomes easier, with practice, than might be expected *a priori;* but even so there are plenty of occasions when the moving pattern of processes is too complex for seizure by an unaided introspection. *O* grasps at the familiar qualities, but is well

aware that there are changes of clearness and of temporal course, integral to the pattern, which he cannot get into the formula of his report. The results of the present study are, in so far, not only incomplete but also (unless this positive correction is made) misleading; they must be supplemented by detailed analyses, made under more strictly experimental conditions, which shall permit a repeated observation of typical chess-patterns under all their distinguishable aspects.

An analysis of the game falls naturally into four divisions. First comes the *preliminary period,* the period just before the game; secondly, the period of the opening, the *opening-game*; thirdly, the middle period, the *mid-game;* and fourthly, the closing period, or the *end-game.* The period just before the game is distinct; the others to a certain degree merge into one another, and the divisions are more arbitrarily made. The following report has been gleaned from the introspections of all the games played blindfold,—games won and games lost, games in which the play has been pleasurable, and games in which it has been very trying.

Preliminary Period

The observer anticipates the game in visual, kinaesthetic, and verbal terms. It is through the peculiar combinations of these imaginal trains that the characteristic quality of "chessness" is obtained.

During the early months of play, the *visual images* consist of a picture of a chessboard, and of glimpses of former games which are static in their presentation and which closely resemble the pictures of problems printed in the newspapers. These static views pass quickly in and out of the focus of attention.

The early introspections show that as soon as *O* turns his thoughts to chess a vivid visual image of a chessboard arises. The image is thin and gauzelike, filmy but not vaporous. It is about two feet square, and appears to stand about three feet before him at right angles to his line of regard. It maintains this relation to the line of regard, no matter how much or to what angle he moves his head or eyes. The squares are colorless. The quality of those which represent the black

squares is gray and translucent; those which represent the white are glassy, transparent and clear, showing through them whatever visual quality is imaged as lying behind the board. The distinction and separation into squares is on the whole rather vague. The number of squares is never clearly apperceived.

No men appear upon the board, and it is only with great effort that *O* can image any of them there. When he does succeed, he can keep them in place only for a short time. The images seem to slide slowly downward and off the board. This visualized chessboard will hereafter be referred to as the vertical board. Its origin may be traced to the first game of 'blindfold' chess, in which (it will be remembered) a second board was used as an aid to the player. The vertical board is much like this board, and occupies the same relative position; and the great effect produced by the first game is evidenced not only by the intensive recurrent images that followed it, but also by the determination of the position of opponent and board of play in all subsequent games. Besides the image of the vertical board, a visual image of the word "chess" frequently arises. (This image is a reproduction of the word that appears in script upon the back of the board on which *O* has from childhood played draughts.) When it is clear, the other visual images are correspondingly obscure.

Sibilant *verbal images* frequently accompany the visual images of the word "chess." They sometimes precede and sometimes follow the visual image. The verbal image is auditory-motor, a long-drawn-out whisper.

The *kinaesthetic images* consist in the projection upon the board of 'lines of force,' which are peculiar to the various pieces of the game, and which weave themselves over the board like a 'cat's-cradle.' The projection takes place by way of strong kinaesthetic images of eye-movements and a "feel" of moving the men. In this complex—the feel of moving the men—the images are restricted to the larger and heavier muscles of arm and shoulders. *O* has no images of grasping the pieces with his fingers. The kinaesthetic images thus differ from those that give the "cardness" to a game of cards. In 'thinking' of a game of cards, *O* finds that kinaesthetic

images of holding cards in the hands and fingers are very strong. In chess, the peculiar and characteristic kinaesthetic images are those centered in the muscles of the shoulders, and particularly in the right shoulder.[1]

Later introspections show a decided simplification in the imagery of this period, all the images being reduced to the kinaesthesis of zigzag movements of the eyes. This is, at rare intervals, supplemented by a visual picture of a board such as appears in the chess-papers; but the elaborate imagery reported in the early games is lacking. For the most part, the meaning of "chessness" is carried by the kinaesthesis just mentioned.

The general *conscious attitude* that prevails during the preliminary period of any game is determined, first, by the character and ability of the opponent as a chess-player. With inferior opponents, *O* usually lacks interest, and has no keen desire to win; but with players of equal or superior strength, his interest and desire to win increase proportionally. His conscious attitude also depends upon his general bodily health. When he is tired, or feeling out-of-sorts, he has very little interest in the game, and plays carelessly and indifferently. When, on the other hand, his physical and mental condition are good, he has a decided interest and plays carefully and thoughtfully. Lastly, the reports of the early and late games show a great difference in *O*'s assurance. At first he fears that he may be unable to carry his game to a close; later on his notes show that he has through practice gained confidence and that he no longer concerns himself about finishing the game. His attitude changes correspondingly from worry to eager anticipation.

The Opening-Game

As soon as *O* is comfortably seated, he focusses his attention upon the game, and orientates himself in regard to the

[1] The muscles of this shoulder are particularly well developed, since *O* had, in his undergraduate days, as a member of the track team, put the shot; and they are also particularly sensitive, as the shoulder had been broken and the ligaments torn in football. These facts may help to explain why the images of the right shoulder bulk largely in his mental life.

board. This mental maneuvering continues until he has adjusted himself stably to the imaginal world. The concentration of attention implies the suppression of irrelevant ideas, and the focalisation of particular complexes. Prominent among these, in the early games, is the visual image of the vertical board, which is carried over from the preliminary period. This image is accompanied by kinaesthesis in the eyes, which follow along the diagonals and coördinates of the board, and by other kinaesthetic images which extend along these lines and which are localized in the muscles of the shoulders. The vertical board does not long occupy the center of consciousness. Even in the early games it soon gives way to a second board, a horizontal board,—that upon which the chessmen rest, and upon which the actual play takes place. A heavy line bisects the image of this board, corresponding to the point where the half boards fold together.

O's attention, then, turns now to the horizontal board. In the early games the transition is gradual; in the later games it is rapid. As this board rises in importance, the vertical board sinks. The squares of the horizontal board are less well differentiated even than those of the vertical. There is frequently a suggestion that certain squares are black, and others red, but it is not derived from visual cues. The squares are the queen's and the two bishops'. The color of the queen's square is retained by association with the phrase "queen on her color"; and that of the bishops' squares by the verbal association "white bishop to the right, and black to the left." Reference to the color of a particular square is unusual, and occurs only occasionally during the course of a game. When the color is desired, however, *O* has to work his way across the board from one of the three known squares to that in question. The process involves visual, kinaesthetic, and verbal images; the most important of these is the verbal image.

The horizontal board is not uniformly localized. Its position varies from game to game, and within the course of a single game from moment to moment. In one of the early introspections *O* reports:

"The board is in front of me; it is horizontal, and it lies directly under the vertical board."

In the introspection of a later game he says:

"I localize the board now in my eyeballs, and now a few inches before me. It is horizontal, and it appears as though I were viewing it as in visual play. It fluctuates back and forth according to the degree of attention given to the game."

The position of the chessmen upon this board always bears a constant relation to *O*. His own pieces are on the side of the board toward him, and his opponent's away from him. There are times, however, when this relation is voluntarily reversed; when (for example) his opponent presses an attack and threatens a mate. Then the game is viewed as if from the opposite side of play. Thus, one of the later reports reads:

"I realized that my king was in great danger of being checkmated, so I voluntarily and with great effort reversed the board, and looked at the game from my adversary's point of view. I did this so that I might consider all the possibilities in making my reply. It required some little effort to keep the board turned, and after satisfying myself with the observation I ceased the effort—let go, so to speak,—and the board then righted itself of its own accord. The board during these processes was visualized within the eyeballs."

As has been said above, it is upon this board that the chessmen are imaged and that the play is performed. The pieces and pawns on both sides of the board are clearly visualized. The visual images of the pieces do not differ; they are of the same shade of grey (slightly lighter than medium grey), and of the same shape. The images of the pawns are also uniform, but they are smaller. The images do not resemble the actual chessmen; they are round, like a checker; in other words, they are like the chessmen when viewed directly from above, the bases alone being seen in contour.

After *O* has thus orientated himself toward the board, and established the attitude for blindfold play, serial and static images of past games, of past openings, and of the results and complications to which certain openings lead, arise in visual, verbal-motor, and kinaesthetic terms. As his attention shifts from one opening to another the visual image of the board changes, and his men

are arranged in every case according to the plan of opening then in mind. There is in the early games a strong tendency to play Ruy Lopez, the first opening learned. Later, however, when *O* has become familiar with Giuoco Piano, the opening play is about equally divided between these two games. Exceptional openings are due either to a desire to play something new, or to some external influence. When he plays against an inferior opponent *O* often forsakes these standard games and plays some less familiar opening. In one game with a superior player he opened with Scotch Gambit, a game that he seldom plays, because he was asked by his opponent: "Are you going to play Ruy Lopez?" The counter-suggestion was stronger than the tendency that led to Ruy Lopez, and he accordingly changed his moves. The moves of the opening game are quickly and automatically made, with a strong visualization at the end of each one.

O's attitude during the opening of the game depends to a great extent upon his attitude in the preliminary period. If he is not interested, or if his opponent is inferior, he gives but little attention to the opening moves, and accepts rather indifferently his adversary's replies; but if his opponent is his equal or his superior, and if the attitude in the preliminary period is that of eager anticipation, his attitude during the opening of the game is strained and expectant. In such instances he is worried lest some reply be made with which he is unfamiliar. In the interval between his opening and his adversary's reply, static visual images of all of the standard moves that he knows pass rapidly through the focus of attention. He is always greatly relieved when his opponent accepts his opening, or makes some variation with which he is familiar.

Mid-Game

The machinery of the mid-game consists of complexes of *visual, kinaesthetic* and *verbal images,* concomitant *attitudes,* and *feelings.* The visual and kinaesthetic images are particularly strong and clear, and are very closely interrelated.

The *visual images* are those of the horizontal board and of the pieces upon it. The images of the developed pieces are very clear and distinct. The other pieces are in the margin of

attention, and have little active part in the game; they are but weakly visualized, and are considered merely as undeveloped resources. On the whole, the position of *O*'s men is more clearly imaged than that of his opponent's. When a piece or pawn is moved from one square to another, the visual images involved are of two kinds. Either the visual image of the piece moves across the face of the board, or the piece occupies the new square without having been visualized in passing. The second of these experiences is the more common. When movement across the face of the board is seen, —and this is usually the case in moves involving long jumps, —the piece seems to be moved by an invisible force. On some occasions, the visual image leaves the surface of the board, travels across, and descends to the new square. The knights, when they are visualized at all as passing from one square to another, always leave the surface of the board. The bishops also leave the surface when they move across their long diagonal. The queen likewise, when she makes very long jumps. The pawns leave the surface of the board only when they are first moved, and then only when they are moved forward two squares. The rooks never leave the surface, no matter how great their move. The king, too, never leaves the board, except in castling, when the visual image of the rook slides upon the surface to the square next to the king, and the king then jumps over and behind the rook, clearly leaving the board in the process.

The force which causes the imaginal pieces to move about the board seems to be vested in them; they are not moved by any external power, such as an imagined hand. When a piece has been captured, the visual image of the capturing piece occupies the new position, and the image of the captured piece disappears entirely; there is no 'graveyard' or place where the captured pieces lie. Here it may be added that *O* never images his adversary; it is much as if the opponent's game were played by the pieces themselves.

The *kinaesthetic images* are very closely knit together with the visual. Although kinaesthesis plays an important part in the game, the complexes are extremely difficult to analyze. As has been stated, the visual images of the pawns and pieces

are alike, and so also are the images of *O*'s men and those of his adversary. They are differentiated by the kinaesthetic images, which consist chiefly of eye-movements, slight movements of the head and of muscles of the face and neck, contractions of the eyebrows, tightening of the muscles of the jaws, wrinkling of the forehead, and contractions of the muscles of the shoulders. The peculiar strength and character of the images localized in the shoulders are clearly brought out in one of the early games. *O* reports:

"While waiting for my opponent's reply I discovered that he could take a pawn with his bishop. As the relative position of the two pieces had not been changed for two moves, and since my opponent had overlooked it once, I was hoping that he might fail to discover it again. There were strain sensations or images (I know not which) in the shoulders, that carried this meaning. In character they were like those in wrestling; it was as if I wanted to interpose my body along the diagonal between the bishop and the pawn."

Kinaesthesis gives a definite character to the visual images, and also to the peculiar pattern of the men upon the board. The lines of force which weave back and forth across the board, between the various pieces, form (as has been said) a sort of 'felt' cat's-cradle. This is comparatively simple in the early stages of the game; but as the pieces are developed the pattern becomes more and more complex. The development of a piece is like the addition of another kinaesthetic string to the 'cradle'; the capture of a piece is like the removal of one of these strings. At the opening of the game, when the kinaesthetic lines of force are comparatively simple, they are all of the same degree of clearness;[2] but when the mid-game is reached, and the 'cradle' becomes complex, the different lines of force are not all upon the same level. The processes

[2] Here is a simple illustration. The situation figured by A. Binet (*Psychologie des grands calculateurs et joueurs d'échecs,* 1894, p. 300, Fig. 17) is 'felt' by *O* as follows: the rooks are held as a line of kinaesthesis running vertically up and down the face between forehead and chin, the pawns are in the right shoulder, the bishop is a tendency to throw the head over to the left, and the queen is a tendency (felt mainly in the left shoulder) to throw the left hand up and to the right. All these feels are components of a single bodily attitude or gesture. It will be seen that their pattern agrees closely with Sittenfeld's visual schema (*op. cit.,* 301, Fig. 18), except that *O* gives the pawns the place of honor (in the right shoulder). This may be accounted for by the

involved in the immediate plan of attack or defense are the clearer. The others lie in the background, from which they emerge and to which they return as the play proceeds.

The *verbal images* appear as soon as the play opens. The moves are transmitted auditorily, and they are retained in memory in verbal (auditory-motor) form. Verbal images are also used in planning an attack or defense. In all cases, however, even in those in which the move is decided from the visual and kinaesthetic cues, verbal images have to be finally employed in the formulation of the words by which the move is transmitted to the adversary. Verbal images seem absolutely necessary also in cases when a move is to be reported, and *O* does not know the position upon which it terminates. For example, in one of the early games verbal images were employed in establishing the square on which the bishop, in Ruy Lopez, is placed on the third move. Visual and kinaesthetic images were closely knit up in this complex, to be sure, but the verbal images were the most prominent. The visual image consisted of the board and of the pieces upon it, the kinaesthetic image consisted of the line of force between the bishop's square and the queen's knight's fifth, the square the name of which *O* wished to obtain. He named in order the squares from the bishop's to the one desired; "Bishop one; King two; Queen three; Queen's Bishop four; Knight five. Move Bishop to Knight's fifth." As these squares were called off, the visual images increased in clearness.

In the later games the verbal images have become more mechanized, and greater use is made of secondary cues. The position of a square is now obtained, for example, by associa-

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fact that the issue of the play which the attitude or gesture represents is to give white an opportunity to queen. See the remarks on *O*'s treatment of problems in Note 5.

The onlooker always remarks *O*'s restlessness during a game. Sometimes the restlessness is of a general kind, a sort of hitching or squirming or twitching; sometimes it takes on, for a little while, the appearance of a definite gesture (shrugging, the swelling of pride, shrinking or wincing, etc.). Movements of the head are very frequent, and facial expression (so far as the bandage allows of observation) is extremely varied. *O* is in general unaware of these movements as such. No objective record was made of them.

tion with the heavy line that runs through the center of the board, where the halves fold. *O* knows without counting that the squares upon the one side of this landmark are on the fourth, and those upon the other side on the fifth. Only rarely is he obliged to go through the long process of counting up from the bottom of the board. There are also a number of other short cuts, such as verbal associations between moves, which are especially clear in the opening of the game.

In time of trouble *O* invariably falls back upon verbal images. When his attention is distracted, when the visual and kinaesthetic images are evanescent, when he feels that he is losing his imaginal memory of the positions, then verbal images are employed to strengthen the game. The procedure —motor and in extreme cases auditory-motor—consists in naming the pieces on the board and in accentuating these verbal images by movements of the head and tongue. Thus, *O* reports in one of the later games:

"The verbal images are particularly strong, and consist in naming the men, and in some instances in naming the pieces and the squares that they are occupying. Slight movements of the head accompany in rhythm the naming. In the case of the pawns merely the name "pawn, pawn" is repeated; but in the case of the pieces, especially those in the center of the board and in the thick of the fight, the piece and its position are named, as: knight, queen four; bishop, rook three; etc."

The verbal auditory-motor images are of the whisper of *O*'s voice, and the verbal motor images are of movements of the tongue and the muscles of the throat.

The *images involved in planning a move* are again visual, kinaesthetic, and verbal. The visual images of the pieces actually shift when the advisability of making a certain move is considered. If the move prove to be satisfactory, the image remains upon the new square; if unsatisfactory, the image returns to its former position. This procedure is continued until a satisfactory move has been decided upon. The kinaesthetic images are closely knit up in the complex, and consist of movements of eyes, head, shoulders, etc. The verbal images are concerned chiefly with the verbal expression of the move, as transmitted to the opponent.

During the interval between his moves and his adversary's

reply *O* is variously occupied, according to his attitude toward the game. When he has little interest in the game, his attention to it is low, and extraneous associations flit through his mind; but when he is interested, and has a desire to win, his attention is constantly upon the game. In this case, he plans the moves that his opponent is likely to make, and the best replies to them. The planning of the opponent's moves is precisely like that of his own, except that the visual images are always returned, and are not permanently shifted until the opponent announces his moves.

The *effect of the opponent's announcement of his moves* varies with *O*'s attitude toward the game. When his attention has been dispersed and his mind filled with extrinsic associations during the interval between his move and his opponent's reply, the announcement of the move brings his attention back immediately to the game. The visual and kinaesthetic images of the horizontal board and of the men upon it are reinstated; sometimes slowly, and sometimes rapidly, according to the degree of distraction. At the same time the adversary's move is repeated, sometimes auditorily and sometimes visually. The repetition helps to bring back the images of the board and men, and also to establish the new move and relate it to the others. Thereafter the visual image of the piece moved appears in its new position.

When *O*'s attention to the game is good, his opponent's announcement of his move is accompanied by a shift of the visual image of the piece in question.

In either case, when the opponent's move has been established, *O* runs over in visual terms[3] all the kinaesthetic lines of force which radiate from the piece in its new position. When its relations have been established in visual and kinaesthetic terms, *O* turns to the formulation of his reply, in the manner described above.

In this period of the game, *O* seems to have run the gamut of *attitudes*. These were not analyzed, but merely reported in the course of play. They may, however, be roughly

[3] *O* does not, under these circumstances, see the lines of force as bands or strings. He sees a vague picture of a coordinate or diagonal of the board.

divided into two classes: intellectual and emotional. Briefly, the more 'intellectual' attitudes are the following: realization, suspicion, hesitation, admiration, recognition, certainty, uncertainty, doubt, difficulty, inattention, trying to remember, that I could if I would, that I have lost the game, that I have made a mess of it, that I have gained a stalemate, that my opponent is taking a long time to move, that the game is over more quickly than I had expected, that it is the same opening as played before, that it is Ruy Lopez, that it is Giuoco Piano, that it is a game with which I am unfamiliar. The 'emotional' attitudes are: impatience, disgust, hopelessness, easiness, uneasiness, exultation, excitement, comfort, unrest, that I have been tricked, nervousness, anger, that I should like to swear, that I may as well give up, that I may as well resign, and that I may as well go on with it.

End-Game

The machinery of the end-game is similar to that of the mid-game, though the pattern and complexes are very much simpler, and the position of pawns and pieces is easier to carry since there are fewer men involved. The kinaesthetic pattern is especially strong, and is reinforced by visual images. Verbal images play a very small rôle, and are chiefly concerned in the formulation and reception of the moves.

The attitudes of the end-game, whatever they may be,—disgust, impatience, uneasiness, easiness, exultation; all emotional attitudes,—are always subordinate to the feeling of relief.

Summary

The mental complexes involved in the game consist of visual images, kinaesthetic images and sensations, verbal motor and verbal auditory-motor images, concomitant attitudes, and feelings. The proportion and relation of these processes vary greatly. In the *preliminary period* of the game, visual images predominate. From them arise the vertical board, and the pictures of past games. The kinaesthetic sensations and images of this period are centered in the heavy muscles and tendons of the shoulders and in particular in those of the right shoulder. The attitudes show a wide variety, and

depend upon the ability of the opponent and the general physical and mental condition of the observer.

The chief characteristic of the *opening-game* is a train of well automatized psychomotor actions which represent the opening moves. Within this train, visual images are strong and clear; they include the horizontal board, the men upon it, pictures of openings, and memory images of past games. The kinaesthetic sensations and images are very closely related to the visual imagery. They give it emphasis and significance. The attitude of this period is usually strained and expectant, owing to the fear that an unfamiliar reply will be made. Verbal imagery appears as soon as the game opens, and consists in auditory-motor images of the words that the opponent uses in transmitting his move, and of the motor imagery which carries the observer's repetition of these words. This verbal imagery, however, is usually not very clear.

In the *mid-game* the visual and kinaesthetic imagery attains maximal strength and clarity. The images are very closely related to one another in moving the men, in castling, and in capturing pieces; also, in the reception of the opponent's moves and in the planning of a reply. Verbal imagery appears, of a different kind from that of the opening-game. It functions in the naming of the men and of the squares upon which they rest. It is especially strong when other imaginal resources threaten to fail. This period is very rich in conscious attitudes.

The *end-game* differs from the mid-game less in the quality of its imaginal and sensational processes than in the quantity and variety of the processes involved. This part of the game is simpler and is carried with less difficulty than the former period.

After the game the observer is able to reproduce the play either 'mentally,' or perceptually upon a board. The reproduction is done chiefly in kinaesthetic and visual terms, although verbal images frequently enter into the complex, particularly if *O* is uncertain what move was actually made at the particular stage of the original game which he has now

reached. The game is unfolded as it was developed. The observer can not tell, for example, what the 14th or 25th move was, until he comes in the regular course of re-play to make that move. The game is in general retained, not as a number of discrete moves, but as groups or connected series of reactions toward more or less well-marked situations.[4]

The average length of the blindfold game has been two hours. This long duration is not due, as might be supposed, to the time consumed by the observer, for in not a single instance did he spend as much time upon his moves as did his opponent. In justice to his adversaries, however, it must be said that they were concerned with other things besides the play, such as the timing and tabulation of the moves, the recording of the observer's changes of attitude, etc. The average number of moves in the thirty-one games played is forty. The average for the games lost is 34, for the games won 45. The shortest game was lost in 21 moves, and consumed an hour and ten minutes; the longest game was won in 69 moves, and lasted three hours and twenty minutes. Of the 31 games played the observer won 14, lost 14, and drew the rest. He gained one of the draws against the most experienced of his opponents through a stalemate.[5]

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[4] If, for instance, at a certain stage of the game a set or group of moves (say, three to five in number) represents a definite plan of attack or a definite reply to a threat, then these moves may be reproduced (other things equal, and the disposition of the board permitting) in *any* order; *O* remembers the total response to the situation, but does not—without special effort, and not always then—recall the precise succession.

Here belongs the further fact that it is in general easier for *O* to play a game blindfold, from its beginning, than to solve a problem from a printed diagram. The game is all of a piece; but the problem presupposes a game already partly played, and *O* has no idea of the course and plan of this game.

[5] Since completing this paper *O* has read Binet's account of blindfold chess (*op. cit.*), and E. E. Southard's report of the blindfold reproduction of a game played in the ordinary way (R. M. Yerkes, *Introduction to Psychology*, 1911, pp. 190ff.). (1) Binet, as was natural under the circumstances, appears to have exaggerated the rôle of *erudition*. For *O* knows much less about chess than the average amateur; he has made no study of openings, and carries only three or four in his memory; and he has not either studied chess-problems.

He has, indeed, no particular interest in the technique of chess, and no patience for problems; either he solves a problem by inspection, in a few minutes, or he drops it. He brought to chess, however, an expert knowledge of draughts, a game in which, even as a child, he was unusually proficient. (2) Binet, prepossessed by the idea of visualization, completely missed the importance of kinaesthesis, though to the reader of today his account suggests it at many points.

(3) Southard appears to substitute for *O*'s kinaesthetic cat's-cradle a single exploratory feel: he "seems to feel something . . . in right upper arm muscles . . . combined with a visual image of a moved dark long object," or has a "combined visual and kinaesthetic image as of some object of a snout-like character moving about in the region of the chessboard in question." It is curious, as the writer himself points out, that the reproduced kinaesthesis is referred to the right upper arm, although the actual move might have been made by the left hand. This reference may be more general than *O* supposed when he wrote Note 1.

(4) A. A. Cleveland (The Psychology of Chess and of Learning to Play It. *American Journal of Psychology,* xviii., 1907, p. 293) remarks that "knowledge of checkers is at first a source of many interferences." *O* found no trace of this associative interference; nor did Cleveland's feebleminded chess-player (*op. cit.,* p. 307)!

VISUAL RHYTHM[1]

By Christian A. Ruckmich, University of Illinois

Introduction

The problem of the existence of visual rhythmisation has frequently presented itself both without and within the field of psychological inquiry. Architecture suggests the problem in the treatment of repeated structures, *e. g.*, the effect of columns in a façade, of exposed timbers in ceilings, of repeated arches in vaultings, and of special patterns, such as ornamental brick designs.[2] Sculpture introduces the problem in the modeling of a frieze, or in the panelling of a wall in bas-relief. Mural painting furnishes examples of the problem in the recurrent *motif* of border decorations. But the main pressure toward a solution of the problem comes from the discipline of psychology itself. The issue is raised on both systematic and on methodological grounds: systematically it is a question of fact while methodologically a new avenue of approach to the inner mysteries of rhythmical perception is open.

Systematically the possibility of visual rhythm has frequently been denied. Probably the narrowest delimitation of the field of rhythm has been made by Külpe in his statement,

> "It is a curious fact that the involuntary rhythmical apprehension of stimuli is confined to auditory impressions."[3]

Ebbinghaus further extends rhythmical perception to the sensations of bodily movement:

> "The impression of unity in multiplicity is not limited to simultaneous sensations; it is also produced—and indeed just as immediately and unreflectedly as in the other cases—through successive sensations, but

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[1] A preliminary report of these experiments was presented at the New York meeting of the American Psychological Association, December 27, 1916.

[2] F. B. Gilbreth, *Bricklaying System*, 1909, p. 265.

[3] O. Külpe, *Outlines of Psychology* (trans.), 1895, p. 389.

essentially, to be sure, through those which can easily be produced successively with sharp demarcations, such as auditory sensations and the sensations of bodily movement. In their simplest form these successive unities are designated as rhythms."[4]

Wundt does not deny rhythm to visual impressions, but he explains its handicap in this field. In the following passage he apparently puts no limitations on rhythmical perception:

"For the sake of understanding all these illusions of length of intervals through indirect temporal ideas, convincing observations of short as well as of long series of beats give evidence that no series of impressions exists which cannot be somehow rhythmically apprehended."[5]

Elsewhere,[6] however, he holds with Ebbinghaus that discreteness of sense-impression is a prerequisite to its rhythmical incorporation. Visual sensations are accompanied by lasting after-effects which, if the rate of impression is fast, fuse the separate impressions into a gradually changing perception of movement, as witness stroboscopic effects. If, on the other hand, the rate is so slow that the after-effects are avoided, then the most favorable rate for rhythmisation is exceeded. Titchener is very definite in his earlier statements:

"There are only two classes of sensation that can form the basis of the perception of rhythm. These are the auditory and the tactual or 'motor'; there can no more be, *e. g.*, a visual rhythm than there can be an auditory symmetry."[7]

But in a subsequent discussion of the subject in another text,[8] after a preliminary consideration of the general field, the possibilities are further restricted. The only true rhythm is kinaesthetic in its essence:

"The author was formerly disposed to attribute a separate rhythmical perception to hearing, but recent observation has convinced him of the existence of kinaesthetic sensations due to the contraction of the *tensor tympani* of the middle ear. . . .[9]

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[4] H. Ebbinghaus, *Grundzüge der Psychologie*, I, 1902, p. 484.

[5] W. Wundt, *Grundzüge der physiologischen Psychologie*, III, 1911, p. 53.

[6] W. Wundt; *Ibid.*, pp. 35-36.

[7] E. B. Titchener, *Experimental Psychology, Students' Qualitative Manual*, 1901, p. 174.

[8] E. B. Titchener, *A Text-book of Psychology*, 1916, p. 345.

[9] It ought to be noted here that Mach at first assumed a special sense of rhythm which he located in the ear and that Ewald explained the intimate connection between the auditory sense and temporal estima-

The perception of rhythm may be aroused by visual impressions, whether by simple series of discrete stimuli, presented under laboratory conditions, or by the sight of rafters on a corridor ceiling, or of the recurring ornaments of a façade. In the author's opinion, this rhythm is always kinaesthetic, based upon eye-movement, upon slight movements which tick off the successive impressions, or upon some other form of intermittent kinaesthesis. Even those writers who believe in a purely visual rhythm acknowledge that kinaesthetic associates are almost invariably present, and are exceedingly difficult to suppress."

Pillsbury seems inclined to credit at least four sense departments with rhythmical perceptions, but in varying degrees:

"Auditory and kinaesthetic impressions show rhythm most easily, tactual and visual, not so markedly."[10]

For Myers the scope of rhythmical perception is not quite so comprehensive:

"The appreciation of regular intervals between presentations or between recurring groups of presentations, is the basis of rhythm. The presentation may be tactual, auditory, or kinaesthetic; they may be similar or dissimilar."[11]

While most of the material used in the text is gleaned from the auditory and motor fields of sensation, Calkins does not commit herself to such a limitation in the statement:

"One is conscious of rhythm in dancing, reading poetry, playing an instrument, and in watching the dance and listening to poem or to music." [12]

Among many of the experimenters in the field of rhythm there is the same tendency to deny or to minimize the possibility of a perception of rhythm on a visual basis. Meumann, one of the pioneers in the investigation of rhythm, in mentioning the problems that still await solution, declared that

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tion on the basis of muscular adjustments in the inner ear. Mach later substituted a special energy of the brain for the theoretical function of the ear. For a detailed discussion *vide* J. B. Miner, Motor, Visual and Applied Rhythms, *Columbia Contributions to Philosophy, Psychology and Education,* IX, 1903, pp. 41-42.

[10] W. B. Pillsbury, *The Fundamentals of Psychology,* 1916, p. 329.

[11] C. S. Myers, *A Text-book of Experimental Psychology,* 1911, p. 301.

[12] M. W. Calkins, *A First Book in Psychology,* 4th rev. ed., 1914, p. 83.

". . . we do not know, why only certain sense-departments give indications of rhythmical phenomena, and others not at all . . ."[13]

But he later concluded that

"each sense-department takes part in rhythmical phenomena in proportion to the degree of its function as an agent of temporal estimation; therefore hearing, which is the temporal sense *par excellence,* is most adapted to the skilful incorporation of rhythmical forms. A much lesser rôle is played by movements which function at the same time in spatial estimation, and a still lesser rôle is played by the visual sense which is demonstrably very weak in the matter of temporal estimation in investigations of the sense of time, and is therefore almost exclusively a spatial sense."[14]

He proceeds to state that we can speak of a rhythm which is induced by successive perceptions of recurrent architectural designs and which affords an experience similar to that of auditory rhythm. This rhythm would then be based upon attentive and ideational changes. To Squire the results of her experimental investigations indicate that

"It [rhythm] appears to be a phenomenon characteristic of but two modalities, audition and movement."[15]

But for Woodrow the problem has a broader aspect for he says:

"To produce an impression of rhythm, it is necessary to have a series of stimuli. These stimuli may be sounds, as in the case of poetry and music, muscular contractions, as in dancing and beating time, or lights and electrical shocks, as in some laboratory experiments."[16]

In similar vein Dunlap remarks that there seem to be no set boundaries for rhythmical perception so far as sensuous content is concerned:

"The facts seem to be that all sorts of sensations lend themselves to serial grouping."[17]

It is significant to note that this country holds two records

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[13] E. Meumann, Untersuchungen zur Psychologie und Aesthetik des Rhythmus. *Philosophische Studien,* X, 1894, p. 249.

[14] *Ibid.,* p. 261.

[15] C. R. Squire, A Genetic Study of Rhythm. *American Journal of Psychology,* XIII, 1901, p. 575.

[16] H. Woodrow, A Quantitative Study of Rhythm. *Archives of Psychology,* No. 14, 1909, p. 5.

[17] K. Dunlap, Rhythm and the Specious Present. *Journal of Philosophy, Psychology and Scientific Methods,* VIII, 1911, p. 350.

for pioneer work in the field of the psychology of rhythm. Bolton, so far as we know, was the first to investigate experimentally the general field of auditory rhythm; and Miner recorded the first experimental evidence for visual rhythm. The entire problem of visual rhythm seemed to lie dormant until the beginning of the twentieth century, exactly a decade after the first experimental attack on the problem of the rhythmical consciousness. In another decade the problem was again investigated by Koffka, of the University of Berlin, who studied the rhythmical effects of visual stimuli which were arranged in definite temporal patterns. Subjective rhythm was also studied. A few years ago the author undertook a short series of experiments with visual stimuli differing objectively only in intensity. All three investigators come to the same conclusion:

"Contrary to the prevailing impression, we have found that the experience of rhythm in the field of vision is identical in its essentials with that in the auditory field. Since the experience is novel, it is at first more vague than with sounds, but it becomes quite precise with practice. Although visual rhythm is less distinct, it is just as direct as auditory. The difference so far as is noted, is in degree, not in quality."[18]

"From this negative aspect no essential difference between auditory and visual rhythm has shown itself. This paragraph has therefore demonstrated to us the fact that series of visual imagery can be the sole associates of the experience of rhythm. At the same time they easily assume, especially in the higher groups of beats, a spatial structure. This indicates that for rhythmical experiences visual and auditory imagery are equivalent throughout. Only to the circumstance that rhythm is constantly experienced in the realm of tone and but seldom in the realm of the visual senses, can we ascribe the fact that the influence of the auditory factors is rated so great and of the visual factors so small."[19]

"Rhythm is demonstrated in the visual sense-department as well as in the auditory."[20]

"We next ventured into an investigation of light-rhythms. . . . All observers perceived a rhythm. The three periods were in contents much what we have described in regard to the other series (auditory), with the exception that kinaesthesis of eye-movements was very marked

[18] J. B. Miner, *Op. cit.*, p. 71.

[19] K. Koffka, Experimentelle Untersuchungen zur Lehre vom Rhythmus. *Zeitschrift für Psychologie,* LII, Pt. 1, 1909, pp. 96-97.

[20] *Ibid.*, p. 104.

in all of the introspections—a result probably due to the extensiveness of the flashes on the screen."[21]

As we scan the literature we detect, then, a tendency on the part both of systematic writers and of the early investigators to rule against the possibility of visual rhythm: Külpe, Ebbinghaus, Titchener, Myers, and Squire, are explicit or implicit in their denial; Wundt, Meumann, Pillsbury, and possibly Calkins, admit visual rhythmisation with qualifications; and Woodrow, Dunlap, Miner, Koffka, and the author admit it to full rank. On the whole, experimental evidence seems to favor the last interpretation.

So much for the situation so far as systematic treatment is concerned: we have still to note the significance of the methodological aspects of the case. This point is clearly made by Koffka.[22] Much experimentation has been done in the last two and a half decades on the general subject of rhythm. Theories have almost equalled these attempts in number, and difficulties have arisen out of all proportion to the facts discovered. The rhythmical consciousness is now known to be much more complex than was at first suspected. Genetically, as Wundt and others have indicated, rhythm is a very old mental complex. This is especially true of the sort of rhythms that have commonly been analysed. But more than one investigator has shown that visual rhythmisation, when it exists, is a comparatively novel experience; and all of the special studies which have been undertaken in this field are at one in pointing out that visual rhythm does not differ essentially from any other kind of rhythm. The question then arises, why not direct the energies of those interested in the general field of rhythm more specifically to the study of visual rhythmisation? As a matter of procedure, the task would seem to offer greater rewards, because of the possibility of studying rhythmisation in its growth. The process would be essentially synthetic; others analytic.

It was with something of this sort in mind that the following investigation was undertaken. The first question was: (1)

[21] C. A. Ruckmich, The Rôle of Kinaesthesis in the Perception of Rhythm. *American Journal of Psychology*, XXIV, 1913, p. 356.

[22] K. Koffka, *Op. cit.*, p. 3.

can visual stimuli be rhythmised on the basis of differences in quality, *i. e.*, color, and (2) what are some of the characteristics of visual rhythm?

Observers

Five observers have taken part in the experiment at various times: *S*, an undergraduate student of advanced standing, *T*, *U*, and *V*, departmental assistants in the first, second, and third year of their graduate work respectively, and *W*, a Fellow with psychology as a major study. The order given indicates approximately in an increasing degree the amount of introspective practice which each observer had had. *S* and *W* observed in only a few of the preliminary series of experiments. The others completed the entire series to the end of the experiment. *T* observed in 63 trials, *U* in 84 trials, and *V* in 81 trials. All of these trials were in addition to a long series of preliminary trials in which the colors were subjectively equated for intensity.

Procedure

The experiment was started in the spring of 1915 and continued at intervals until the middle of the second semester of 1917. Most of the material gathered is from the final series which was begun early in the academic year of 1916-1917. Our first task was subjectively to equate the differently colored lights for intensity. This problem was partly complicated by the fact that changes of intensity, through insertion of variable amounts of electrical resistance in the circuit of the lamps, would be accompanied by changes in saturation and, in the case of the yellow light, also by changes in the hue. Yellow became orange. This problem was finally solved by using lamps of different wattage and with varying amounts of lacquer. The other factor which complicated the problem was the inadequacy of several of the standard methods for the determination of the equality of brightness. We first tried out the method of equation in indirect vision, but we came to conclusions similar to those of Rand.[23] Equations

[23] G. Rand, The Factors that Influence the Sensitivity of the Retina to Color. *Psychological Monographs*, XV, 1913 (No. 62), pp. 1-166.

obtained in peripheral vision did not hold for the same observers in direct vision under similar conditions of adaptation. After computing long series of equations with the method of flicker-photometry, we were again unsuccessful. Equations for red and yellow would hold, but not for red and blue or for yellow and blue.[24] Other difficulties arose when observers tended to judge not in terms of brightness but of apparent size of stimulus. Moreover, at the point of the disappearance of flicker the brightness of the light was much below the effective working conditions of the experiment. The colors assumed such a poor degree of saturation that they elicited an unpleasant affective response. Our final appeal was made to the 'equality of brightness' method as used in direct vision in accordance with the psychophysical method of limits. This method not only proved successful with each individual observer, but a point of equality was reached which was common to all observers. The three colors were taken in pairs and the brightness of one member of the pair was gradually modified through the introduction or the elimination of resistance until the point of subjective equality was reached. The resistance values for the several lights were then kept constant throughout the experimental series.

With this important problem settled, we then arranged a series of temporal patterns in which the flashes were to appear. At the time that observations were taken E would set the apparatus for the given pattern in accordance with the program outlined on a sheet of procedures, reduce the overhead illumination in the observation room to the approximate value of an 8 c. p. light in order to avoid the formation of after images, give two flashes of the fixation light as a 'ready' signal, and after an interval of four seconds throw the switch which connected the lights with the control apparatus. Each flash lasted .5 seconds and was followed by a .5 second interval. In some patterns one flash of light was periodically omitted giving an interval of 1.5 seconds between groups. The total time of a single series amounted to 30 seconds on the average,

[24] *Vide* C. E. Ferree and G. Rand, A Preliminary Study of the Deficiency of the Method of Flicker for Photometry of Lights of Different Colors. *Psychological Review,* XXII, 1915, pp. 110-162.

the length of the period depending somewhat on the type of pattern because care was taken to end the series at a point analogous to the point of origin. Toward the end of the experiment a few trials were given at a faster rate in order to add an element of confusion, but such a series was exceptional. At the close of a series the illumination in the observation room was restored to normal and the observer wrote out his introspections in accordance with the following instruction:

> A series of colored flashes will be produced at the fixated area. Report the perception and give an analytical account of your conscious processes during the period.

Twenty-two series, with several variations of color in each case, were given. For purposes of record they were designated by Roman numerals.[25] The composition of the series is indicated by the row of Arabic numbers which is appended to each serial designation. The Arabic numbers give the temporal disposition of each of the several colors, the dashes represent the omission of flashes giving therefore an actual physical interval, and the dots indicate a continuation of the preceding series. Although a record of the colors which were used was kept, the colors are not noted in the table because on repetition of the series the position of the colors was altered.

I	1111.........	XII	11-11-.........
II	1212.........	XIII	123-123-.......
III	123123........	XIV	111-111-.......
IV	122122........	XV	121-121-.......
V	112112........	XVI	122-122-.......
VI	121121........	XVII	112-112-.......
VII	12221222......	XVIII	13231323......
VIII	11121112......	XIX	13221322......
IX	11221122......	XX	123321123321..
X	12111211......	XXI	123231123231..
XI	12-12-.........	XXII	133231133231..

It will be noted that, with the exception of the preliminary series I, and of series XII and XIV, the flashes differed only

[25] In the discussion of the results the observers are indicated in italics followed by the number of the trial in the experimental series. At the end of the introspection the nature of the series is given with the colors designated by their initial letters, thus (series XV): (RYP-).

in hue. It will be remembered that the flashes were objectively equal in duration, and had been subjectively equated in intensity. In series I conditions were analogous to those which induce subjective accentuation in experiments with the metronome. In series XI-XVII the group was objectively distinguished by the lengthening of the interval between flashes from the customary .5 seconds to that of 1.5 seconds.

Apparatus

The experiments were conducted in two isolated dark-rooms in the Psychological Laboratory of the University of Illinois. In the one room, the more secluded of the two, sat the observer; in the other, the experimenter's room, was located the control apparatus with its accessory appliances. The two rooms were connected by a door which was closed while the experiments were in progress. It is convenient to describe the two rooms separately.

The Observer's Room

The observer sat in a comfortable chair with his head in an adjustable headrest. He was provided with an electric push-button for the purpose of signaling the experimenter in the event of any accidental mishap or at the close of his written introspective report. The instruction was written out and fastened to the desk-arm of his chair. Opposite the observer and at a distance of 2.5 meters from his eye, a wooden box, 19 cm. wide, 27 cm. high, and 21 cm. deep, was located. The box[26] contained a circular opening, 1 cm. in diameter,[27] situated in the middle of the side opposite the observer and approximately at the level of his eye. The opening was carefully cut in a cardboard slide so that no fringe or rough edges would be exposed. Behind the slide a small piece of 'opal'

[26] An attempt has recently been made by C. F. Lorenz, of the Nela Laboratory, to apply the principle of rhythmical flashes of colored lights in the advertising field. He inserts a number of 'flashing' lamps of various colors in a large translucent globe. Since both the duration of the flash and the intervals between flashes are variable in each lamp, no constant rhythm can be maintained for any great length of time; but the effect of grouping would be interesting to study.

[27] The size of the opening used and the conditions of illumination in the observer's room were the same as those in Koffka's experiments.

or 'milk' glass was fastened. Inside and as nearly opposite to the opening as physical conditions would allow, four incandescent electric lamps were mounted on the back of the box: a 40 watt mazda lamp lacquered in blue, another 40 watt mazda lamp lacquered in red, a third 10 watt mazda lamp painted in white enamel, and a 4 c. p. carbon lamp used only for the preliminary 'ready' signals. Between and directly behind the lamps and opposite the opening of the box, a bright tin reflecting surface was inserted. Trials were made to determine whether the observer could locate from the appearance of the flash the position of the lamp which corresponded to that color. Even when this judgment was made with some degree of confidence—and that was rare—the results were not at all consistent with themselves or with the physical conditions to which they referred. The box was lined with white Bristol board on the inside and was painted a flat black on the outside. It was also wrapped with cloth to prevent the leakage of light. The room was lighted by means of a 40 watt mazda light and reflector. The illumination was controlled from the experimenter's room, so that during the period of experimentation the lamp could be dimmed to the equivalent of an 8 c. p. lamp by throwing a 60 watt carbon light in his room in series with it. This process served as a first preliminary signal and during the experiment prevented the formation of halos (Hering's *Lichthöfe*) and after-images.

The Experimenter's Room

Electric connections from the first three lights mentioned passed through suitable switches in the experimenter's room to a control apparatus which was actuated by the clockwork of a Ludwig-Baltzar kymograph of the improved form. The fourth lamp in the box was operated by the experimenter to indicate the position of the fixated area and to serve as the 'ready' signal. On the drum of the kymograph, which was burnished to provide good electrical contact, perforated strips of heavy shellacked paper were mounted. The function of these strips was similar to that of pianola records, for together with a series of multipolar switches, which determined the color of the lights themselves, these strips gave the temporal

pattern in which the colored lights were to appear. The perforations were made with a small hand-punch and were uniformly 3 mm. in diameter. They were punched in three rows on coördinate paper at 3 mm. intervals in the direction of the rotation of the drum. A standard placed near the drum carried three small hammered brass contact-springs which passed over each of the three rows of perforations and through them made contact with the metal body of the drum, thus completing the circuit of the lights in the box. The sound made by the passage of the springs over the perforations was muffled by winding heavy silk cord about the springs. Very heavy felt was also placed beneath the kymograph to deaden the hum caused by its rotation. The springs were connected to a series of multipolar switches by means of which any row of perforations might be connected with any one of the three differently colored lamps. Any row could be disconnected by setting the appropriate switch at the neutral point. In series with each of two of the lamps a standard resistance-box of 11,110 ohms was inserted. No resistance was needed for the blue light because it proved to be the least bright of the three. It was possible, however, to connect the resistance-boxes with any one of the three lamps, a procedure that was rendered necessary in the preliminary trials when the points of the subjective equality of the respective brightness values were determined. As the result of these tests the red lamp-circuit carried a resistance of 200 ohms and the yellow lamp-circuit carried 280 ohms. In the main series the 110 volt service-main supplied the alternating current for the lights. An auxiliary kymograph and timing fork were also on hand for recording the duration of the stimuli which were presented. These readings were taken from time to time as a check on the rate at which the flashes were being delivered. The adjustments on the kymograph were so made that the duration of a single flash was exactly .5 seconds.[28]

[28] This seems to be a favorable length and is endorsed by several investigators in the field. C. H. Sears, A Contribution to the Psychology of Rhythm. *American Journal of Psychology*, XIII, 1902, p. 33; C. R. Squire, A Genetic Study of Rhythm. *Op. cit.*, p. 504, 575, who holds that from .3-.6 seconds is the most natural rate for 'free' or bodily rhythms.

Results

In view of the experiments that have already been recorded by others and by the writer in the field of visual rhythm, it is no longer necessary to prove that visual rhythm is a possibility. Our results, as far as they go, tend to reaffirm this possibility and to fortify the conclusion that in the visual field rhythm may be induced in terms of attributes similar to those which obtain in the auditory field. Auditory rhythm may be based upon differences in the duration, in the intensity, or in the pitch of the members. It has been shown that visual rhythm may be induced in terms of differences in the duration of members,[29] in the intensity of members,[30] or in the temporal arrangement of members.[31] Our first task, then, is to demonstrate that, after having equated the members of a series for both of these quantitative attributes, observers consistently report rhythmical experiences when the stimuli differ objectively only in a qualitative aspect. On this point our results give a definite answer. Introspective evidence warrants the statement that rhythmical perceptions may be induced by visually presented stimuli which differ only in color.

In a preliminary series (A), 30 flashes of a single color were given at intervals of one second. The flashes did not physically vary in duration, in intensity, in color, or in duration of intervals. We have from this series the following typical reports:

S-6 "Red color. It seemed to come in rhythmic form, the second two of a three-series being close together."

V-2 "Blue color. Feeling of confusion in terms of general kinaesthesis: I wasn't sure whether I was seeing a series of three or of four." [There were also differences in the "apparent intensity of stimulation."]

These reports are the more surprising because we had not expected to get evidence of visual rhythmisation from this series. Our aim was to proceed gradually toward the problem of a visual rhythm based solely upon differences of color

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[29] J. B. Miner, *Op. cit.*, p. 71.
[30] C. A. Ruckmich, *Op. cit.*, p. 356.
[31] K. Koffka, *Op. cit.*, p. 104.

without giving the observer any indication of the nature of our experiment. It is only fair to state that the other observer upon whom this series was tried out failed to report any grouping. These results indicate, however, that at least by some observers, the same sort of 'subjective rhythmisation,' which is a common phenomenon of auditory series, is experienced also in a visual series.[32]

From the simplest of our series, alternate flashes of two colors, to the most complex patterns which we presented (series XXI and XXII), rhythmical groupings were recorded by all of our observers. Occasionally conditions prevented the formation of a consistent or definite rhythm, but in general rhythmical perceptions were the rule rather than the exception. Even when, toward the end of our investigation, we made the pattern quite complex and increased the speed of presentation without warning, almost without exception rhythms were reported. The following quotations were selected at random from the protocols:

S-3 "One, two, three, was the rhythm." (BRR)

T-14 "A very pronounced rhythm was present from the first. The two blues came very close together while the interval between the blue and the red was long." (RBB, reported as BBR)

T-48 "The series came in groups of four as given. The blues were accented during the first part of the series but later only the first blue was accented." (RBYB)

U-25 "A pattern was formed at once into blue, short interval, red, long interval." (BR)

U-48 "The pattern, yellow, red, blue, was formed at once. The first color, yellow, appeared slightly longer in duration and was, perhaps, a trifle brighter; the intervals seemed also a little longer after yellow." (YRB-)

U-70 "The series was formed after the first few flashes—just as soon as two yellows had appeared together I came to the conclusion that blue, red, yellow, yellow, was the series." (BRYY)

U-80 "This whole series formed one single pattern." (BRRYRB, reported as BBRRYR)

V-20 "This seemed decidedly to have the rhythm of about waltz time, but the blues came in too slowly." (RBB)

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[32] This agrees with the results of Miner who finds that on the basis of duration: "The experiments show rhythm to be subjectively experienced from a series of like lights." *Op. cit.*, p. 71. *Vide* also Koffka, *Op. cit.*, pp. 26ff.

V-58 "The pattern came at once, definitely and clean cut, in visual sensations and verbal kinaesthesis. The darkness of the tints of red and blue seemed to make them fit in well, for the emphasis given them in kinaesthetic strains. The rhythm was pleasant because easy to grasp." (RYBY)

V-75 "The pattern was perceived as rhythmical; much more regular than any of the others." (RBYBYR)

W-1 "Kinaesthetic sensations or images and auditory images gave a certain rhythm which was practically the same for each group of three." (RYB)

W-9 "Kinaesthesis in throat—which was nearly as clear as the visual processes—and somewhat vaguer auditory images gave the rhythm." (BR)

Direct introspective statements to the effect that the perception was rhythmical were supplemented by repeated reports of certain secondary phenomena which are familiar to the investigators of rhythm. Accentuation and grouping induced characteristic changes in the attributive relations of the members of a series. The duration of members suffered most change. Observer *U* reported such variations in 29% of his records, *V* in 14%, and *W* in 100%. As a rule the accentuated member was longer than any of the others. In the words of one observer:

U-36 "Giving the first red a duration of 3, the other durations would have been roughly: red 3, interval 2, blue 2, interval 3, blue 1, interval 1, blue 1" (RBBB).

Another important factor is the matter of subjective intensification of members. This effect always accompanies the accent. *U* reports it in 16% of his introspections, and *V* in 7% of her introspections. Changes in the hue, tint, or chroma of visual sensations are likewise mentioned in 6% of *U*'s reports and in 9% of *V*'s reports. In addition *U* mentions differences in size in 9%, variations in shape in 3%, and alterations in affective tone of the members in 13% of his reports. *T* also mentions differences in affective appeal in 16% of his introspections. More uniformly than any of these effects was reported the lengthening of intervals between groups. *S* mentioned this in 40% of her reports, *T* in 29%, *U* in 20%, and *V* in 10%. Both *U* and *V* described this interval as a 'pause' or 'rest.' Occasionally this interval was related to changes in attentional clearness of processes:

U-41 ". . . I tried to find what the 'rest' which came after each 'pattern' consisted of and I noticed a shifting of eye-movements which required more time due to a change of color and which was the same as that between a change of colors during the series, such as that between yellow and blue. To this time required for a change of eye-movements was added additional time due to a 'lapse' or 'relief' of attention. (BYBB)

There seems to be no question, then, in the minds of observers that in general throughout the experimental series their experiences were of a rhythmical order. With this conclusion before us, our next step is to show that we obtained *visual* rhythms. Before proceeding, however, we must come to terms with the expression, *visual rhythm*. When may a rhythmical perception be properly called *visual?* As far as practice goes—and, of course, it does not yet go very far—*visual* rhythm is an expression used to designate a rhythmical experience induced by stimuli that are visual. In like manner, are the expressions *auditory, organic,* and *tactual rhythm* used when the principal field stimulated corresponds to the adjectives given. In this sense, surely, we have appropriately used the phrase. All of our observers agree in the statement that while the rhythm was being formed visual sensations were the most prominent processes in consciousness. In other words, the rhythm was largely a visual perception. Statements to this effect follow:

S-5 "A series of different *colors,* grouped in threes." (BRY)

T-10 "Attention was fixed directly on the light." (RY)

U-42 "A pattern . . . for which the 'color' appeared to be the cue." (RYRR)

V-28 "Yellow, yellow, yellow, red—the same throughout. This was in the foreground of attention." (YYYR)

W-8 "Visual sensations were much the clearest processes in consciousness." (RB)

In giving these citations we do not claim more than the bare fact that the rhythmical perceptions arose from stimuli which were visually presented, and that corresponding visual sensations were attentively experienced. It is hardly necessary to add that processes other than visual are normally present in complex experiences of this sort. Indeed, in many studies of auditory rhythm the prominent rôle of kinaesthetic factors has repeatedly appeared. Under certain circumstances, such

as the initial comprehension of a very difficult rhythm, the motor factor may even be reported as the most prominent item at a critical moment of a rhythmical perception. And there may be other factors as well: the rhythmical perception is an exceedingly complex affair. One is reminded of an analogous statement usually attributed to Rowland that the structure of an atom is as intricate as the mechanism of a grand piano! So it is with rhythm. The grouping effect of a rhythm in any case may depend on visual patterns, on auditory imagery, on organic complexes, on changes of clearness, on alterations of temporal arrangements, on verbal ideas, on motor responses, and on many similar items. But the point to be emphasised here is that all of these items are integrated about a nucleus of *visual* sensations, of *auditory* sensations, of *tactual* sensations, or of *kinaesthetic* sensations; and inasmuch as this occurs, these complexes can be legitimately designated respectively as *visual, auditory, tactual,* and *motor* rhythms.

But we may go a step farther. We may ask, as we did in a previous study: is it possible to induce a rhythm which shall be perceived principally in terms of those sensations, or of their attributes, that correspond directly to the nature of the stimuli presented? In other words, in this particular set of experiments is there evidence for the statement that a purely visual rhythm can be experienced as such? On the basis of our results we strongly incline toward an affirmative answer. The reports of practised observers indicate that the grouping effect, which may or may not be accompanied by accentuation of a member of the group, can be carried almost, if not entirely, in visual terms, *i. e.,* without the influence of such additional aids as kinaesthetic sensations. Two of the three observers who were retained until the end of the experiment definitely reported such instances: *T* in 16% of his reports, and *V* in 24% of her reports. The third observer did not specifically mention such experiences, but frequently stated that some qualitative or quantitative attribute of the visual sensations gave the key to the nature of the group, which in turn was maintained in terms of eye-movements. Character-

istic descriptions of these occurrences are transcribed from the protocols:

T-27 "A visual image at the beginning of the series determined the course of the whole series. The image was prompted, however, by the series. The two lights seemed to take the form of a great swing and move from left to right. . . . There was a swing from blue to red and then a quick jerk back to blue, thus bringing the red and blue close together. The image of swinging in a swing as I used to do was very vivid but was accompanied by no kinaesthetic sensations. The swing was of large amplitude." (RB-)

T-37a "The spatial arrangement was the prominent complex during this series. The lights appeared in a series of stairs. In the early part of the series the stairs were regular, having a certain number of steps. Later the number of steps became variable, but always the red took the forward direction and the blue the up and down direction. The sensations were grouped together in pairs, thus, RB, RB, RB, *etc.*, the red being accented. The interval between the red and blue consequently seemed shorter than between the blue and the next red." (RB)

V-45 "The second exposure was perceived as rhythmical and with it came a vivid visual image of a V-shaped figure, with two reds at top and blue at bottom. I think this perception of rhythm came entirely in visual terms, with an intensification of the reds subjectively by fixating attention thereon and by sliding over blue. I did name them each time in verbal-kinaesthetic terms, but no one was particularly emphasized. The presence of the red at the top of the V-pattern meant more stress thereon. Once in the middle of the exposure period the blank between the two series was perceived as a rest; this came in verbal-kinaesthetic terms. This perception came so entirely differently from any other that it was accompanied by a feeling of wonder or surprise, in chest kinaesthesis and slightly in shoulders, meaning, 'well, isn't that funny,' I believe I actually perceived a rhythm visually." (RBR-)

V-72 "Blue, red, red, yellow, red, blue. Verbal kinaesthesis determined the pattern almost entirely at first, by stressing. Then kinaesthesis continued but in the background, and visual processes became very clear. Every other color beginning with the first was perceived as stressed because darker. With this there was a visual image of the pattern [a zigzag line with B,R,R at top and R,Y,B at bottom]; the colors were not so closely assimilated, however, into the image as usual. All through the period the first hues of the series were carried over in visual images to the end of the series." (BRRYRB)

Introspections of this sort were given much less frequently than those which specified the emphatic presence of kinaesthetic factors. But the occurrence of rhythmical patterns in

purely visual terms is a possibility if only a single example is indicated. To the writer and to several of the observers who have since expressed themselves, the evidence that in the sense outlined above rhythm can be experienced in visual terms seems conclusive.

We pass on to the consideration of further results. We have already spoken of the presence of kinaesthetic factors. As has been discovered in many other experiments on rhythm, we found that, in point not only of frequency of occurrence but of the importance of the part played, motor factors are almost indispensable items in the rhythmical consciousness. In one form or another they were recorded by all of our observers. *S* spoke of kinaesthetic sensations localised in her hand in 40% of her reports; *T* mentioned verbal-kinaesthetic sensations of naming colors and of ocular movement in 74% of his reports; *U* referred to the same sort of kinaesthesis in 68% of his introspections, but of the total 60% were reported as kinaesthesis of ocular movement; *V* described the rhythmical perception in terms of kinaesthesis, most frequently that of counting or of naming the colors, in 87% of her reports; and *W* mentioned kinaesthetic factors in all of her introspections. The roll call shows a unanimous and decidedly expressed vote. As in our previous investigation of the rôle of kinaesthesis in rhythmical perceptions, our present study of the introspections reported indicates again that "when the rhythm is first heard, kinaesthetic strains, muscular contractions in head, throat, neck, and the other parts of the body, appear as references for the interpretation of the rhythm and as aids to its clear perception."[33] In many of the reports we find that kinaesthesis centers about the problem, or the *Aufgabe,* and is therefore most prominent at the beginning of the period of introspection. A few extracts from the protocols may serve to make this clearer:

T-11 "At first there was a vague attempt at a vocal accompaniment, this shading into a verbal repetition of the appropriate nouns and then nothing remaining but the visual image of the colors in position as represented. . . ." (RBY)

T-24a "There was an interesting confusion at the first. The series began with kinaesthetic sensations in the throat and in the eye, but

[33] C. A. Ruckmich, *Op. cit.*, p. 351.

they soon seemed to be carried as an echo to the perception. . . . The confusion mentioned above lasted but a few seconds." (RRYY)

U-78 ". . . The pattern formed very slowly at first and was confusing—carried along in auditory-kinaesthetic processes. When the pattern was discovered—which in this case was not done entirely passively but there was an attitude and an effort to find a pattern—there was a decided change in auditory-kinaesthetic (verbal) imagery and in eye-movement, both becoming regular and more pleasant than they had been." (RYBBYR) [In the case of observer *U* whenever the series is difficult to grasp verbal kinaesthetic sensations of naming the colors are called into play. Otherwise the series runs off in terms of ocular movements and visual imagery of spatial arrangements.]

V-37 "Yellow, yellow, and just a bodily feeling that a long time was elapsing, localised in chest kinaesthesis. Then came the meaning 'yellow, yellow, blank, blank.' This came in verbal-kinaesthetic terms, and was followed by a general kinaesthetic relaxation of having solved the problem. After that attention fluctuated from the fixated area to other perceptions and ideas. But during the whole period, I kept repeating the series in verbal-kinaesthesis quite passively and in the background. . . ." (YY-)

V-44 "Blue, blue, blue. At first there was a general kinaesthetic feeling of having the *Aufgabe* of determining the rhythmical pattern. This soon dropped out and I took the exposures passively. . . ." (BBB-)

V-66 ". . . Then the pattern went off so easily that nothing seemed to be clear except visual processes, with verbal kinaesthesis in the background. The red was perceived as darker and that gave it the emphasis for the rhythmical pattern. The rest of the period flowed off smoothly in the same way, with mild kinaesthetic set in the background, meaning pleasure and satisfaction." (RBBB)

Almost every observer furthermore recorded changes in affective tone as the period progressed. While the problem remained unsolved or while interruptions in the way of rearrangement of groups occurred, consciousness would invariably be unpleasantly toned. Near the end, however, if the rhythm had been grasped, the affective tone changed to pleasantness. Of course not all rhythms were experienced as pleasant even when the task had been accomplished, but that depended on the nature of the rhythm itself. At such times indifference might be reported.

The three observers who took part in the main series of the experiment showed marked individual differences in the method of carrying the rhythm. Some of these differences have already appeared in other parts of this report. A sum-

mary of these differences was submitted to each one of the observers at the close of the entire experimental series and with slight modifications was approved as read. The summaries follow:

Observer *T*

(1) The nature of the rhythm may be determined almost entirely in terms of verbal kinaesthesis of naming the colors and rarely of counting them, or in terms of kinaesthesis of ocular movements which for the most part seemed similar to the sensations of ocular accommodation. These sorts of kinaesthesis may not only form the basis of grouping, but of accentuation. But even more frequently visual images of spatial position and of movement may determine both the group and the accent.

(2) Rarely do secondary factors play a part in rhythmisation. No differences in duration or in intensity were reported and only once was hue mentioned as a determining factor. Somewhat more frequently the affective value of the color played a part.

(3) Auditory imagery, especially images of pitch differences, were very frequently reported (58%).

(4) Many reports of alterations in the durations of intervals were given. On two occasions there was evidence of the disappearance of intervals.

(5) Occasionally visual or auditory processes carried the rhythm without the presence of kinaesthesis.

Observer *U*

(1) The nature of the rhythm is determined almost entirely in terms of ocular movements which frequently give definite positions to the lights in a visualised schema. This sort of kinaesthesis may be supplemented by verbal kinaesthesis of pronouncing the name of the color, which sort of supplementation is especially prominent whenever the rhythm is difficult.

(2) There are secondary factors which also play an important part in the rhythmisation. For the most part they are attributive to the visual perception itself. In the order of frequency they are: duration, intensity, affective coloring, size, quality, and shape. Frequently the accent of the rhythm may be carried in terms of these secondary factors, especially in terms of duration, quality of color, intensity, and size.

(3) Only a single auditory image of pitch difference was recorded.

(4) There are also many indications of the distortion of intervals both between members and between groups. The second class of intervals was peculiar in that it represented qualitatively different intervals which were frequently designated as 'periods of rest.'

(5) There was no evidence of a clearly visual rhythm, *i. e.*, a rhythm perceived visually without the aid of kinaesthesis.

Observer *V*

(1) The nature of the rhythm may be determined almost entirely in terms of verbal kinaesthesis, especially that involved in counting. This determined the stress. Frequently, however, visual images give the colors spatial positions which not only determine the group but also the accent or stress.

(2) Very much less often secondary factors also play a part in rhythmisation. In general these factors are attributive to the visual perception itself. In order of frequency they are: duration, quality, and intensity.

(3) No auditory images of differences in pitch were recorded. [The observer remarks the general rarity of auditory imagery in her experience.]

(4) Occasionally also the duration of intervals suffers distortion. There are a few records which indicate a disappearance of intervals entirely. Intervals between groups, which were often reported as 'pauses,' were largely the result of changes in organic complexes that accompanied changes in the visual pattern.

(5) A large number of purely visual rhythms were reported; others were reported in which kinaesthesis was present but played only a minor part.

There are other phenomena which deserve attention. In the first place, there were many characteristic illusions. In addition to the usual illusions of length of intervals, and of duration, intensity, and quality of members, which have already been mentioned, there were some reports of a change of rate in presentation. The rate increased, or decreased, as the series progressed, or the rate of the series was declared slower or faster than the preceding. In most of these cases—and they were reported by all observers—it seemed that tempo was dependent on affective value: if the series presented an unusual amount of difficulty, it was unpleasant and appeared slow. If the difficulty was overcome in the course of the trial, a pleasant affective tone ensued, and the series seemed to proceed faster. Another phenomenon which is frequently found in auditory rhythms came quite unexpectedly to light. I refer to the matter of 'inner grouping.' In the course of a trial the group would often break up into smaller groups: a six-rhythm would become a double three-rhythm or a triple two-rhythm; a four-rhythm would become a double two-rhythm, and in

one curious case a three-rhythm! *T* reported this effect in 5% of his reports, *U* in 8%, and *V* in 15%.

Summary and Conclusion

Collecting our results together and viewing them as a whole, we may make the following general observations:

(1) Rhythmical perceptions based upon visual stimuli lend themselves unusually well to critical inspection. The writer is satisfied that, in his experience with the two sorts of investigations, the study of auditory rhythms on the one hand and the study of visual rhythms on the other, more direct evidence can be gained and more promise of solving some of the ever-recurrent problems is derived from experiments on visual rhythm.

(2) It is possible to obtain rhythmical perceptions from stimuli that are visually presented and that differ objectively only in color quality. It is furthermore possible to obtain from such stimuli experiences of rhythm which are visual in their very essence, *i. e.*, in which no other processes play an important part. The main difference between this sort of visual rhythm and the common experience of auditory rhythm lies in the fact that the former is much less frequent in its appearance and that there are marked individual variations in the ability of observers to perceive the rhythm. These variations appear to be more marked than the usual distinctions between rhythmical and unrhythmical observers.

(3) The kinaesthetic factor is a large item in the rhythm consciousness, but in this also individual variations occur. It is evident that the chief office of the kinaesthetic factors is related to a carriage of the instruction to comprehend the rhythm at which time it is clearest in consciousness; later in the period, if it persists, it may continue to represent the accent and the group, in which case it usually recedes into the background of consciousness. The particular kind of kinaesthesis varies with the observer, but for the most part the sensations were localised in the throat and about the eye. It is probable that ocular-kinaesthesis was particularly prominent in these experiments on account of the nature of the stimulus. In the writer's mind the Wundtian doctrine of an affective basis in the dimension of *Spannung und Lösung* rests upon an ulti-

mate substructure of kinaesthetic and organic sensations which were frequently reported as alternating through periods of strain and relaxation with each group of the rhythmical series. And with them often came changes in affective tone as well.

In spite of the prominence of these kinaesthetic factors the writer is not inclined to agree with Titchener in regard to the ultimate nature of the rhythmical experience, on the ground that observers report as the clearest part of their perceptions the sensation and images which correspond to the stimuli given. The essential part of the perception in every case consists of the visual sensations with their attributive changes and their concomitant alterations of intervals. Upon these processes primarily depend the accent and the group. Kinaesthetic processes are important but ancillary to the rhythmical perception.

(4) It does not seem correct to say that either the group or the accent is characteristically carried in a certain way. The more the reports were analysed the more indications were at hand that at different times different factors enter without the slightest consistency for observer or type of rhythm. Accent was described as given by the quality, size, shape, intensity, of the sensation, by the position of visual imagery, by intensity of any accompanying processes, frequently of the kinaesthetic processes, or even by auditory imagery. To the writer the group appears to be a complex of perceptions organised in terms of imaginal and kinaesthetic processes on a basis of affectively toned organic processes. The function of the ideational processes in this complex must not be underestimated because it is largely in these terms that the members which have passed still appear to be experienced and the ones to come are anticipated. Whatever additional physiological effects may accrue it is difficult, of course, to say, but no doubt some tendency persists which carries the group on.[34]

(5) In general many of the phenomena which accompany other kinds of rhythm manifested themselves. Intervals were under- and over-estimated; attributes were subjectively assigned to the members; subjective rhythmisation occurred; and redistribution of groups was common.

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[34] *Vide* K. Koffka for a discussion of the perseverative tendency in this connection. *Op. cit.*, pp. 47, 65, 74.

TEMPORAL JUDGMENTS AFTER SLEEP[1]

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The ability of a person to wake in the morning, either at a regular time or at some specifically predetermined hour, has sometimes been assumed to depend upon a judgment made in sleep. To explain such a judgment, some writers have posited an unconscious cerebration during sleep, while others, who regard judgment as necessarily a conscious function, have argued from the facts of predetermined waking to consciousness during sleep.[2] Now our knowledge of the mechanism of the temporal judgment in waking life does not lead us necessarily to suppose that there is during sleep a continuous consciousness or a continuously functioning 'unconsciousness.' On the contrary, waking under a determination may occur in response to some particular cue. In the morning the possible cues from outside sources are numerous. Experiments upon waking at prearranged times in the dead of night would, therefore, show more clearly the extent to which environmental stimuli are essential; but, even should these environmental factors prove irrelevant, there remains the possibility of cues which depend upon the organic state of the sleeper. Such potential cues might, if the sleeper were awakened, acquire conscious representation for him, so that he might then be able consciously to form a judgment of time by reference to them. Thus an experiment, in which a sleeping subject is wakened and asked both to form a judgment of the time and to record the conscious cues upon which he bases his judgment, might lead to an understanding of this temporal mechanism. If, further, it could be shown that recorded conscious cues constitute an adequate basis for the

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[1] From the Cornell Psychological Laboratory.

[2] *Cf.* C. M. Child, Statistics of "Unconscious Cerebration," *American Journal of Psychology*, V, 1892, p. 249; M. de Manacéïne, *Sleep*, 1897, pp. 32ff. See also O. Külpe, *Outlines of Psychology*, 1895, p. 451.

temporal judgments actually made, we should need to go no further. A second problem would lie in the question whether the basis for these temporal judgments were also the basis for the judgment implied in the ability of the sleeper to wake at a predetermined time. This problem would require a second experiment,—a test of the subject's ability to wake at given times. If the error of waking in this second experiment were comparable to the error of estimation in the first, we might be willing to assume that the cues were the same.

Our paper deals with the first problem only, namely, the accuracy of the estimations of time after sleep, the nature of the designated conscious cues, and the adequacy of these cues to the temporal judgments.

Our observers were Mr. H. G. Bishop (*B*), assistant in psychology, Mr. G. J. Rich (*R*), fellow in psychology, and ourselves (*L* and *E*).

Our plan was to wake the observer at an unknown time during the night, and have him, after waking and turning on the light, record in a note-book (1) his estimate of the time, (2) as completely as possible the conscious cues upon which he based his judgment, and (3), after the judgment and the report had been entered, the actual time. Previous to retirement the observer had recorded the number of the experiment, the date, and the time of retiring. The report called for was obviously not an introspective report, since the observer was to seek for conscious bases *relevant* to the judgment, that is to say, was to give information of particular meanings rather than descriptions of mind. All the observers took this attitude naturally enough, although *R* did at one time worry over the non-introspective nature of his protocols. The reports in this form also generally indicated subjective certainty. In requiring a record of the actual time we intended to dispose our observers toward improvement with practice. As a matter of fact, no such improvement seems to have occurred.

The procedure was *with knowledge*. A haphazard series of fifty settings of an alarm clock was prepared,—we used ten settings of the clock ranging at half-hour intervals from 12:15

to 4:45 and each setting was repeated five times. The limits were selected for the autumn months (October, November in Ithaca) so that all the settings might occur after the usual time of going to bed and before the first signs of dawn. The observers knew that these settings were to be used and were told to limit their guesses to them. The unit of judgment is, therefore, the half-hour.

B and *R* slept in the same room. A friend set the alarm for them every night. Since they were within hearing of a clock which struck the quarter-hours, the alarm was always set (with their knowledge) at 20 or 50 minutes after the hour, instead of 15 or 45. The times and the judgments are recorded in this paper, however, as if they had been for even quarter-hours. These observers lived in a locality where the secondary criteria both of passing automobiles and of crowing roosters sometimes, but not often, interfered. Both kept late hours, so that the methodical arrangement of the experiment was vitiated by their missing some of the earlier times. There were other failures, as well, when they slept through the alarm or fell asleep without writing the report. Altogether *R* reported 45 times out of the fifty; *B*, but 37 times. Because of his irregularity and because he was, of all the observers, the least easily aroused from sleep to the intellectual task of the report, *B* proves to have been the least informative subject. His cases weight the quantitative results less than do those from the other observers and his reports furnish less information.

For *L* and *E* a neighbor set the clock by a haphazard series different from the one used for *B* and *R*. *L* and *E* occupied the same room. They live in a quiet neighborhood where there were never any outside disturbances. They both completed the full series of 50 reports. Unfortunately, errors in setting the clock made the series slightly asymmetrical. *E* awoke more completely than did *L* and furnished the most extensive reports of any of the observers.

As the experiment worked out, the actual number of observations made at every one of the ten times by every observer is as follows:

	L and *E*, each	*B*	*R*	Total
12:15......	4	1	2	11
12:45......	5	3	5	18
1:15......	4	4	5	17
1:45......	7	3	4	21
2:15......	4	5	5	18
2:45......	5	2	3	15
3:15......	5	5	5	20
3:45......	7	6	6	26
4:15......	4	4	5	17
4:45......	5	4	5	19
Total......	50	37	45	182

Although this warping of the method has created difficulty in the quantitative treatment of the results, it has proved to be less serious than has the failure to control the times of retiring. In a repetition of the experiment it would be of value to have the number of judgments made equal for certain given intervals between retirement and the time of making the judgment (*cf.* Table IV).

TABLE I

SUMMARY

Observer:	*L*	*E*	*B*	*R*	Av.: all O's	Av.: all cases
Constant error: mins........ ("+" = too late)	+11	+28	+4	+24	+16	......
Average error: mins.........	52	41	50	58	50	50
Weighted average error: Per cent chance error.	.49	.45	.54	.64	.53	.52

Individual differences appear in Table I.

The constant error for every observer is the average of the constant errors for every setting of the alarm. It is necessary to average all the errors of an observer in order to obtain a representative tendency, since the procedure with knowledge disposed the observer for different errors at the different times. Thus, if a judgment of later than the actual time be regarded as positive, it is not possible for a negative error to occur at 12:15, the earliest time, or a positive one at 4:45, the latest time. Only for the middle times are the tendencies for positive and negative errors approximately equal. If,

now, the errors for all times were averaged together, these factitious tendencies should cancel each other, provided an equal number of judgments were made for every time. Since, however, this latter condition was not fulfilled, we averaged the errors for every time and then averaged these averages, thus weighting all times equally. It appears that all observers show positive errors, increasing in the order *B, L, R,* and *E*. *E*'s error appears, from his reports, to be largely due to a misinterpretation of cues. In spite of his theoretical knowledge that sleep is deepest very early in the night, he assumed that it would get deeper, instead of lighter, for quite a considerable time after retiring; thus, when he based his judgment upon the depth of the sleep from which he had been awakened, he tended to estimate the time somewhat too late. Even *E*'s error, however, is not large with respect to the unit of the experiment, thirty minutes.

The average errors (Table I) in minutes are averages of the errors for all cases taken without regard to sign or to the times at which they occurred. The observers' knowledge, however, renders this mode of treatment questionable. If the judgments were mere matters of chance, the average error would be greater for the extreme times (12:15 and 4:45), where it should be half the distance between the extremes, than for the middle times (2:15 and 2:45), where it should be about one-fourth the distance between the extremes,—assuming always that in a chance judgment all times are equally probable. Under this assumption, the average chance error for every time would be as follows:

12:15 and 4:45,	135 mins.
12:45 and 4:15,	111 mins.
1:15 and 3:45,	93 mins.
1:45 and 3:15,	81 mins.
2:15 and 2:45,	75 mins.

An error for a middle time is thus almost twice as significant as an equal error for an extreme time. This discrepancy can be eliminated by expressing the errors as *per cents* of the chance error. These average errors we shall call *weighted average errors*. Thus, in Table I, the weighted average errors are averages of the absolute errors weighted separately for the particular time at which every one was made. As a matter

of fact, the weighting makes less difference in the final results than might be expected. Comparison of the weighted and unweighted average errors of Tables II, III, and IV shows a high degree of correlation. The rank orders of the averages, for example, are almost the same. The weighted values in general, however, bear out more positively the conclusion indicated by the unweighted values,—a further reason for granting their validity.

With regard to Table I, we may point out that the average errors vary from 45% to 64% of the chance error, and that the observers rank in order of decreasing accuracy: *E, L, B, R*. The degree of mental alertness on being awakened appears, according to the reports, to descend approximately in the order: *E, L* and *R, B*. *E*'s small error may, therefore, be due to his alertness. The greater part of it consists probably in his constant tendency toward positive error resulting from his specious cue of depth of sleep. Thus an error smaller

TABLE II[3]

TEMPORAL COURSE OF THE AVERAGE ERROR (PRACTICE)

Successive decades of judgments:	I	II	III	IV	V	Average: all cases
Average error: mins.						
Observer *L*	45	39	60	54	63	52
Observer *E*	75	27	27	45	33	41
Observer *B*	39	42	63	60(7)	..	50
Observer *R*	48	60	72	54	54(5)	58
Average: all cases	52	42	55	53	49	50
Weighted average error: per cent of chance error						
Observer *L*	.43	.39	.45	.50	.69	.49
Observer *E*	.81	.29	.28	.52	.35	.45
Observer *B*	.40	.48	.62	.69(7)	..	.54
Observer *R*	.45	.72	.83	.57	.60(5)	.64
Average: all cases	.52	.47	.55	.56	.46	.52

[3] In this and all succeeding tables the averages of all cases are given. These averages are not the same as the averages of all observers. They avoid undue weighting when the number of cases is very different for different observers.

even than that given by any of our observers might possibly have been obtained if, for example, *E* could have learned to correct consciously for this tendency. (For a decrease in the size of the errors resulting from a corrected reinterpretation of the cues, see page 277. Even as they stand, the errors are, on the average, only half the chance error. It appears, therefore, that the judgments cannot have been merely chance judgments, and that some cue, conscious or unconscious, must have been operative.

There is no evidence of *practice*. Table II shows the course of the average error for successive groups of ten judgments. The observers did not profit by their experience. *E*, it is true, made his worst errors in the first decade of judgments. *L*, *B*, and *R*, on the other hand, show a slight decrease in accuracy. There is no constant trend to the average of all the cases. We may explain this lack of improvement in part by the observers' drowsiness. They were less interested in noting the amounts of their errors and in trying to improve than they would have been in an experiment in day-time. Although they frequently referred to previously reported cues, no one ever compared his estimated accuracy of a present judgment with that of an earlier one.

TABLE III

AVERAGE ERROR AT DIFFERENT HOURS OF THE NIGHT

Time	12:15	12:45	1:15	1:45	2:15	2:45	3:15	3:45	4:15	4:45	Average: all cases
No. cases: all O's	11	18	17	21	18	15	20	26	17	19	
Av. error: mins. Av.: all cases	33	75	54	60	45	36	45	39	30	63	50
Weighted average error: per cent of chance error											
Observer *L*	.22	.59	.87	.63	.20	.48	.89	.29	.19	.53	.49
Observer *E*	.11	.70	.48	.78	.60	.24	.67	.32	.13	.27	.45
Observer *B*	.22	.35	.42	.63	.96	.20	.37	.64	.40	.62	.54
Observer *R*	.56	.92	.58	.93	.56	1.08	.37	.42	.32	.49	.64
Av.: all cases	.24	.68	.58	.74	.60	.48	.56	.42	.27	.47	.52

The question arises whether accuracy of judgment depends upon the time of night. Table III indicates a general tendency for the maximal errors to occur from 12:45 to 3:15, with a mode at 1:45 and a minimum at 12:15, which is approximated at the other extreme at 4:15. But the average curve, when plotted, is irregular; while the individual curves are too irregular to give more than a most general indication. Turning from these to other data, however, we find the error a true function of the time elapsing after going to bed. As the hour of retiring was not controlled, the time of night bears only an approximate relation to it. Thus Table III exhibits an imperfectly masked correlation, which becomes evident when the errors are grouped according to the intervals between going to bed and being awakened, as they are in Table IV.[4]

TABLE IV

AVERAGE ERROR IN DIFFERENT HOURS AFTER RETIRING

Individual averages based on 5 cases or less are placed in parentheses.

Hour after retiring	1st	2nd	3rd	4th	5th	6th	Av.: all cases
No. cases: all O's	16	36	33	51	27	14	
Average error: mins.							
Observer *L*	(6)	57	69	42	27	(72)	52
Observer *E*	(0)	57	72	39	27	36	41
Observer *B*	24	63	(21)	54	(105)	(30)	50
Observer *R*	96	54	57	42	(60)	(90)	58
Average: all cases	36	58	63	44	39	51	50
Weighted average error: per cent of chance error							
Observer *L*	(.05)	.53	.76	.51	.28	(.54)	.49
Observer *E*	(0)	.55	.80	.47	.30	.26	.45
Observer *B*	.24	.77	(.26)	.55	(.77)	(.27)	.54
Observer *R*	.85	.60	.72	.46	(.47)	(.66)	.64
Average: all cases E+L+R+B	.32	.62	.72	.50	.36	.39	.52
Average: all cases E+L+R	.35	.55	.76	.48	.32	.41	
Average: all cases E+L	.02	.54	.78	.49	.29	.37	

[4] In this connection we may note that *L*, *B*, and *R* tended to regard their estimates as judgments of the time elapsed since retiring. *E* tended to make absolute judgments, although in some cases he consciously took account of the time of retiring.

The number of hours after retirement approximates the number of hours of sleep. This relationship holds least accurately for *B,* who was fitfully wakeful. In Table IV we have grouped these errors by hours. The column headed "1st" gives averages of errors made less than one hour after retiring; the column headed "2nd," averages of errors during the second hour; and so on.

If we plot the average function for all four observers combined, we see that the curve of error starts at a minimum (average error = 32%), rises to a maximum (72%) for the third hour, then drops again practically to the original minimum (36%, 39%) in the fifth and sixth hours. The curve suggests that amount of error is partly a function of depth of sleep. The objective determinations of depth of sleep (intensity of stimulus necessary to wake the sleeper) indicate that sleep is deepest from one to three hours after its initiation. Bentley reports that of a total of 44 dreams two occurred during the first three hours, four in the fourth hour, and twelve in the fifth hour.[5] Our finding is, therefore, exactly supported by Bentley's and by certain of the objective determinations.[6] It is furthermore consistent with the informational reports of our observers. In deep sleep, apparently, the organic factors which stand for depth of sleep are less frequently supplemented by other temporal cues; thus a large error is apt to arise when these organic factors lack the secondary support that they receive in lighter sleep. We shall discuss these various conscious cues presently.

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[5] M. Bentley, The Study of Dreams, *American Journal of Psychology,* XXVI, 1915, p. 200.

[6] See, for example, E. Michelson, *Untersuchungen über die Tiefe des Schlafes* (Diss.), Dorpat, 1891; S. de Sanctis and U. Neyroz, Experimental Investigations Concerning the Depth of Sleep, *Psychological Review,* IX, 1902, p. 254; and the references and citations in these papers and in Manacéïne, *op. cit.,* pp. 30ff. The time of deepest sleep varies, according to different investigators, from the first hour (Kohlschütter) to late in the second hour (Mönninghoff and Piesbergen); the divergence appears to be due to individual differences in the subjects. Michelson shows some curves ("winter sleep") which correspond to our curve of error with the maximum about the third hour. The slight tendency of our curve to rise in the last hour may indicate a second mode in the curve of sleep; *cf.* Michelson, de Sanctis and Neyroz.

The average curve, however, does not do justice to all the facts; there are individual differences. The individual curve for E is of the general form of the average, though more extreme. It runs 0, 55, 80, 47, 30, and 26% for successive hours. *L*'s curve, except for a rise in the last hour, is practically the same as the average, so that the mean of *E* and *L* (2, 54, 78, 49, 29, 37%) represents a type in which the error increases rapidly from almost zero to a maximum and then falls off gradually to an intermediate value. *R*, on the other hand, makes his maximal errors in the first hour and these fall off gradually thereafter (85, 60, 72, 46, 47, 66%). The difference between *E* and *L*, on the one hand, and *R*, on the other, lies only in the initial stages, since the courses for "$E+L$" and for "$E+L+R$" coincide in the second hour and thereafter. (Table IV.) Whether *B* belongs to the one or the other of these types is not easy to decide. Two-thirds of his results, influenced by his irregularity of retiring, fall in the second and fourth hours. His other averages are therefore based on too few cases to be reliable. The three values which we may reasonably accept indicate that *B*'s type is the same as *E*'s and *L*'s. Hence the average curve for all observers ("$E+L+R+B$") is the same as the curve with *B* excluded ("$E+L+R$"). (Table IV.)

On the theory that amount of error indicates depth of sleep, we must assume that *E*, *L*, and probably *B* do not fall asleep readily and that they arrive at deepest sleep gradually, whereas *R* drops almost immediately into deepest sleep. There is some hint of this difference in the reports. *B*, although very 'sleepy,' was also 'restless' and 'wakeful;' *R* was 'sleepy' in the sense that he slept soundly. *B* reported more dreams than *R* or *L*. Both *L* and *E* 'went to sleep slowly.' At least the existence of individual differences does not work against the hypothesis that the magnitude of the average error is dependent upon the depth of sleep.

The reports give us information concerning the nature of the conscious cues upon which the temporal judgments were based. For the purposes of exposition we may classify these cues under seven heads. Table V gives the number of occurrences of every cue under these rubrics.

TABLE V

FREQUENCY OF THE VARIOUS CONSCIOUS CUES

Number of instances in which every factor was reported as positively relevant and as positively irrelevant; and the total number of instances in which the factor was mentioned as relevant, irrelevant, or without indication of relevancy.

		Conscious Cues						
Observer		1. Excretory	2. Digestive	3. Bodily state	4. Associative	5. Secondary criteria	6. Intervening consciousness	7. Attitudinal or automatic
Relevant	L	8	13	24	1	5	9	..
	E	22	9	32	34	3	15	5
	B	..	..	9	1	7	5	8
	R	12	..	33	13	1	2	6
	Total	42	21	98	49	16	31	19
Irrelevant	L	9	3	..	..	..	..	..
	E	13	7	2	..	..	..	..
	B	..	..	..	..	..	..	..
	R	4	..	..	..	..	..	..
	Total	26	10	2	..	..	..	..
Total times mentioned		77	41	113	49	16	34	19

1. *Excretory Cues.* "Bladder sensations" are mentioned in one-half of the reports of *L*, *E*, and *R*. *B* never refers to them. *R* sometimes couples them with need for defecation. In frequency of mention they are second only to 'general bodily state,' but the function of these excretory cues in the temporal judgment seems equivocal. Forty-two times they are given as positively relevant to the judgment; twenty-six times they are definitely stated to be irrelevant; nine times the observer doubts their relevancy.

In the early trials observer *L* reported bladder sensations, but denied their relevance. "No tendency to judge by bladder sensations." "No tendency to go by bladder sensations, though I note them as fairly marked." "Tried to follow cue of bladder, but it is no guide." "Bladder sensations are decided, but don't seem significant." After the eleventh trial,

however, she alternates in her use of the vesicular cue. Sometimes we read: "Bladder not strong; therefore not late." "Asleep about four hours; bladder full." "Awakened fairly easily; this is one cue; another is bladder sensations." In general, the bladder cue, when it is a cue at all, seems to operate only approximately and to be used as a check upon other tentative judgments; for example: (after the judgment, 2:15.) "I certainly do depend largely on feeling of general rested condition. Yet bladder is fairly full, so it may be later." Doubt of the relevance of this cue is indicated by such a report as "bladder sensations fairly strong *when noticed,* but otherwise they are not prominent." Toward the end of the experiment, *L* often mentioned the bladder factor without attempting to state whether or not it was relevant to the judgment.

E mentioned the bladder in two-thirds of his reports and designated it as relevant in three-fifths of these cases. He found it irrelevant oftener in the early trials than later. *E. g.,* after a 2:15 judgment, he reported: "I have just realized prominent bladder sensations. They seem as if they ought to make it at least 4 o'clock, but I still stick to my guess;" the guess was correct to thirty minutes. "Bladder sensations quite intense; but I did not notice them under the time *Aufgabe,* only when I remembered I ought to observe them." Similar reports appear in several places. On the other hand, the vesicular complex may be consciously adopted: "Bladder sensations . . . are used by me arbitrarily as a cue. They do not seem naturally to mean time." Yet later: "Bladder sensations are strong now, so it must be late;" "Bladder, indigestion, and sleepiness say late;" after mentioning other contributory cues, "Bladder far along; therefore I guess as late as possible." Like *L, E* uses the bladder only as a rough cue in conjunction with others; for example: "It's not early, since there are no associations with last evening. And it's not late, since I was too sound asleep; it was too hard to get waked up. It's the very dead of night. Since 2:30 would be early and 4:00 would be late, I'll say 3:15. Bladder sensations are strong and agree with 'not early.'" "Light bladder sensations may help to keep it from being too early." "I have been awake

once since going to sleep. Details of the evening come freshly to mind. Further I am motorly inert, by no means ready to get up; and perhaps also my bladder is not as full as usual yet. Hence I judge it early; 2:15 fits the conditions about best." Thus the bladder cues, even when utilized, are apt to be relatively unimportant, and often of doubtful significance.

R came to the experiment from a preliminary series of similar experiments performed in the preceding summer. His judgments in these preliminary trials were based almost always on bladder sensations and the feeling of fatigue. In the present experiment he reports excretory cues twelve times and denies their relevance when present four times. This frequency is much less than that in the preliminary experiments, and is accompanied by an increased dependence upon another cue. His accounts are similar to the reports of *L* and *E:* "There is slight bladder pressure; cannot say whether it is relevant or not;" "Some bladder pressure, but I noticed it only after I had formed the judgment;" "Bladder pressure played a very important part to-night, causing me to change to an hour later an estimate made on other bases;" "Some rectal pressure which acts as a cue to time;" "Bladder and rectal pressures are very prominent, yet they do not seem to be the whole basis of the estimate."

2. *Digestive Cues.* *L* and, to a less extent, *E* were subject to indigestion and sometimes used alimentary cues. These cues were of the same rough, secondary kind as were the bladder cues. They were in effect estimates of the degree to which 'indigestion' had advanced during the night. Frequently they were used with the vesicular cue to supplement more definite bases. A few quotations will indicate their nature.

L reports: "This 'degree of indigestion' is a cue, *i. e.* the dark brown taste in the mouth is never bad when you have been asleep only a short time." "Mouth feels only slightly coated, *i. e.,* not advanced far." "Late. Bad taste in mouth shows advanced indigestion."

E writes: "Indigestion-feeling and dark brown taste are well advanced, although I did not turn to them naturally." "Both bladder sensations and indigestion are strong now, so

it must be late." "Basis seemed to be bladder sensations and indigestion sensations; not very intense, hence not very late." "Bladder and indigestion sensations present, but they seem irrelevant." And so on, with nine reports of relevant digestive sensations against seven reports of irrelevant.

3. *Cues from the General Bodily State.* The kind of cue most frequently mentioned by every observer is the kind that reflects the general organic state: fatigue, restlessness, restedness, muscular inertness, sleepiness. Although all observers relied greatly on some such cue, they differed individually in the particular forms which it took.

E bases two-thirds of his judgments upon his muscular inertness. "It's the 'dead of night,' which is, I think, my own organic deadness, the stupidity and inactivity of coming from sound sleep." "Dead asleep, and didn't understand what the alarm meant at first; therefore it did not seem as if I could have been recently awake, *i. e.*, at least 3:15." "Judgment based on extreme sleepiness and 'deadness,' *i. e.*, the time and effort for me to arouse myself to the task of reporting." "It doesn't seem so very late. I think this 'seeming' is perhaps the depth of sleep (motor inadequacy, deadness of my body), a vague impression, but very immediate as a basis." "Seems late, since I was especially sound asleep; on the other hand I was too sound asleep for it to be almost morning." "I woke up easily and became motorly alive readily; I knew at once what the alarm meant; hence not deeply asleep;" and in conjunction with other cues *E* judges that light sleep means that it is late, rather than early. The statement, "Don't feel near to the motor adequacy for getting up, so it can't be late," occurs frequently. In general, *E* uses the cue of organic inertness more often as a positive indication of the middle of the night than he uses its absence as indicative of early or late hours. Of course absence of inertness is a less adequate cue, since it means equivocally either of the extreme times. We have already seen that *E*'s large constant error is probably to be explained by his incorrect use of this cue. He took inertness to signify the middle of the experimental period, whereas the deepest sleep seems to have occurred for him somewhat before the middle. There is thus

more chance that the absence of inertness means the rested condition of early morning than that it points to the transient period of light sleep prior to the deepest sleep.

L also reports the cue of inertness. "I feel absolutely tied to the bed and inert; basis of judgment is largely this organic state." "Very sound asleep; I feel heavily sleepy. I think I judge partly by these general bodily organics." "Sound asleep and hated to wake up; so 'comfy;' I judge of time by this sleepiness." "So sound asleep that I feel sure it can't be late morning." "Very sound asleep, which usually means about three hours after retiring." "Fairly sound asleep, but waked easily; this is one clue for thinking it late."

L finds an equally useful bodily cue in the feeling of 'restedness,' which indicates the amount of sleep, rather than the depth, and thus indirectly the time. It is the antithesis, apparently, of fatigue, and not of muscular inertness. Although the two bases run together in the reports, they appear, meaningwise, to be absolutely distinct. Inertness-restlessness indicates depth of sleep, and thus, by way of the temporal function of the depth of sleep, the time. Fatigue-restedness indicates the amount of sleep, and thus, since the amount is a function of the duration, the time.

L's reports of the feeling of 'restedness' are represented by the following. "Feel only slightly rested. Hence early. I tend to judge on the basis of this restedness-feeling." "Judgment based on usual criterion: degree of restedness." "Don't feel very rested; it must be early." "Judge I have been in bed about three and a half hours; feel to that extent rested." And so on. *L* uses this cue twenty times to nineteen uses of inertness. In fifteen cases both cues were used together.

R relies principally upon a cue of 'sleepiness' or fatigue, which probably belongs functionally with the 'inertness' of *E* and *L,* although its content is more specific and definitely localized. *R* reports this cue in three-fourths of his trials. He gives it as the only operative cue in one-third of the trials, and as occurring alone with a closely related associative cue (see below) in one-fifth of the trials. With the bladder sensations, it was the prominent cue in the preliminary series. As an indicator of the depth of sleep, it is less equivocal than

was inertness for *L* and *E*. It will be remembered, *R* appears to have lapsed at once into sound sleep. Sleepiness should for him, therefore, mean 'early,' and its lack, 'late;' light sleep probably never means 'early.' "The only basis for my guess is the feeling of 'sleepiness' about my head. This feels as if I had not been in bed very long. The 'feel' consists of 'thickness' of the head and general sluggishness of consciousness." "Not very sleepy on awaking, which is cue to time being quite late." "Guess based principally on feeling of sleepiness; no pressure on forehead." And so on again and again with remarkably little change. Sometimes, though not often, *R*'s account suggests a more wide-spread organic basis: "The only possible basis which I can find is the readiness with which I wake up, my liveliness on awakening, which were taken as symptoms of my degree of sleepiness." "Estimate based entirely on the rapidity with which I awoke. I did not feel dazed or sleepy, and there was a positive feeling of activity."

B's 'sleepiness,' like *R*'s, constitutes a specific organic cue. He uses the heaviness of his eyes as a basis. "My eyes still stick, as if I had been in deep sleep." "It is only because I felt so sleepy when the alarm woke me that I came to any conclusion about the time." "It seems that I have been in bed a long time. My eyes feel strained." "Kinaesthesis in my eyes and in breathing are prominent in my estimation." "It seems as if I had been asleep only a little while, because I don't feel so extremely sleepy. [We have suggested that *B*, with *L* and *E*, belongs to the class of those who go to sleep slowly.] My eyes don't stick at all."

4. *Associative Cues.* In addition to the organic sensations in the head, *R* includes in the feeling of sleepiness certain other factors, which may be called associative in the sense that they have to do with the conscious ideational course. *R* describes it as "the clearness of consciousness," "the clearness of my head," "the sluggishness of consciousness," "my mental sluggishness," "slowness of thinking." This factor acts with the organic factors in sleepiness as a cue: "It is the relative clearness of consciousness on awaking that is the cue. Consciousness gets clearer the nearer the time of waking is to morning, or rather the further it is from the time of

retiring." "On awaking my head seems very clear; practically no sensations from it. Also consciousness seems to work with its usual daytime ease. This all means that it is quite late in the night."

E discovers an important 'associative' cue in the logical content of his ideas. On waking in the early night his thought naturally runs on from the topic of the preceding evening, especially from the topic in mind at the time of retiring. The failure of these meanings to come up indicates that the night is at least well begun. If considerable effort is necessary to recall the topic voluntarily, then presumably it is late indeed. In the 'very dead of night' there is often a lack of the usual orienting meanings. *E* reports that he "can not make out what the alarm means," a state which may correspond to *R*'s "sluggishness of consciousness." Toward morning there seems occasionally to be a definite disposition toward the anticipated events of the coming day.

In the early night *E* frequently reports in this manner. At 125 minutes after retiring: "Events on going to sleep seem recent; clear, full, and vivid in memory." After 75 minutes: "This is a strength-of-impression judgment, *i. e.,* going to bed seems clear, facile, detailed; but not so clear and facile as to be very recent." After 45 minutes: "Consciousness seems almost continuous. I think this is a lack of surprise and a readiness of association; *i. e.,* everything fits together with immediately past events." After 90 minutes: "Immediate impression is that I have been asleep only a very short time, for the going-to-sleep images are immediate and vivid. I finally decided, however, that I was hazy enough about the going-to-sleep ideas for the time to be about one o'clock." After 30 minutes: "Sound asleep, but I woke right up into the associative context out of which I went to sleep. Must be very early." After 135 minutes: "It still seems close to last evening and various topics recur, of which I talked. But there is already some blotting out."

Late in the night the ideas of the evening do not readily recur. After 195 minutes in bed, *E* reports: "Vague memorial consciousness of obscure content; going to bed seems far in the past." After 285 minutes: "Not early, since I was too

sound asleep, and there was too little conscious relation to my going-to-bed, which seems long ago and not freshly impressed or strongly connected with the present, *i. e.,* those ideas do not come up more readily than do other ones." After 225 minutes: "Neither late nor early; the going-to-sleep associations are weak; I went over to the topic of the evening only after delay."

We are now in a position to see how the associative cue supplements the cue of bodily state. *E* interprets deep sleep as meaning the middle of the night. Light sleep may mean either 'late' or 'very early;' but light sleep supplemented by the facile recurrence of conscious meanings entertained on retiring indicates early time unequivocally, whereas the absence of these meanings in light sleep points to the late extreme. Taken together the two cues are very important and are used in 46 of *E*'s fifty cases. *E* uses them alone without other bases 17 times.

L and *B* imply the presence of similar cues in many of their reports, but they fail, with one exception each, to give a definite informative statement.

5. *Secondary Criteria.* The judgments were nearly always reported as based entirely upon subjective cues. In 9% of the cases, however, environmental conditions were taken into account, not always with the effect of reducing the error. *L* mentions temperature of the room (three times), hearing a clock strike, and expectation based on her memory of the preceding times in the series (twice). *E* notes the sound of the radiator, and the light outside (moonlight, which he took to be dawn). *B* is influenced by the noises of automobiles (twice), a wagon, a train, roosters (twice), and a clock in the house; also by a memory that in the past he had tended to overestimate the time at night. These cues frequently proved misleading. *R*'s only instance of a secondary cue was the failure of his room-mate to have come to bed at the time the alarm sounded.

6. *Intervening Consciousness.* Frequently (17% of all cases) a judgment was influenced by the memory of a long period of consciousness or of several conscious periods after retiring. The observer would recall that he had been a long

time in getting to sleep, that he had been awake several times since going to sleep, or that he had dreamt much. In such cases he tended to estimate the time as late, or at least as not early. There are six reports among all observers in which this cue is the only one mentioned. Intervening dreams are frequently thus influential. There is, on the other hand, only one recorded case (*E*) in which the occurrence of dreaming at the time of the alarm was taken to indicate light sleep and hence early morning.

7. *Attitudinal Cues and Automatic Judgments* occurred frequently. In reading the reports—especially *B*'s—one can not always tell whether the failure of the observer to report a conscious basis means that the judgment was automatic or whether there was a basis in conscious attitude which, under the conditions of partial somnolence, defied description. When conscious attitudes were definitely reported as such, it often seemed as if the observer, in a more alert condition, might himself have reduced the attitude to some of the other cues. We can not, therefore, presume far on the basis of these immediate, unanalysed judgments. We have listed in Table V the nineteen cases in which the observers stated positively that their judgments were determined in some such manner. Eleven of these cases may be called attitudinal; six (for *B* and *R* only) were reported as immediate and non-conscious.

A question may be raised concerning the adequacy of these various cues to the temporal judgment as correlated with the magnitude of the error. In Table VI we show for four ranges of magnitude of the weighted average error the percentage of the total instances in which each factor was mentioned positively as a cue. For example, there were for all observers 52 instances of errors between 0 and .24. Of these 52 cases, 25% involved excretory cues, 14% digestive cues, and so on. In general, the frequencies of the different cues are the same for different magnitudes of error, and, we may add, this relationship holds in most cases when similar tables are made out separately for every observer. There is a slight indication that large errors tend to occur only in the absence of digestive cues, associative cues, and the cues dependent upon intervening consciousness. There can be no doubt

TABLE VI

Frequency of Relevant Conscious Cues for Different Magnitudes of the Weighted Average Error

Average error in per cent of chance error. Frequencies in per cent of total instances occurring for every range of average error. Based on all cases from the 4 observers.

Range of weighted average error	Conscious Cues							
	1. Excretory	2. Digestive	3. Bodily state	4. Associative	5. Secondary criteria	6. Intervening consciousness	7. Attitudinal or automatic	Total number of cases
0-.24.......	.25	.14	.52	.33	.12	.25	.14	52
.25-.49.......	.24	.14	.61	.31	.08	.20	.10	51
.50-.74.......	.19	.11	.42	.17	.11	.11	.14	36
.75 and over..	.23	.09	.58	.21	.05	.09	.05	43

that the associative cues are mentioned most often when they operate positively, that is to say, when the ideas present on going to bed recur immediately and completely, and consciousness appears logically continuous; and this cue has a high degree of validity. Similarly, the occurrence of intervening consciousness is a reliable indicator that the night is far spent. The use of the cues of bodily state is so general that we find them with the large errors as well as with the small. We must not forget that Table VI deals only with the relative adequacy of these various cues; we have already shown that they are on the average adequate to the temporal judgment to within one-half the error which would occur with pure chance.

We have seen that the cues may represent the course of certain conditions during the night and that certain cues are more adequate at particular times than at others. Since the observer is set to give relevant information, we might expect that the relative frequencies with which a cue is reported at different intervals after retiring would reveal any variation in its adequacy during the night. Table VII, which is analogous in form to Table VI, gives the frequency with which

TABLE VII

Frequency of Relevant Conscious Cues Occurring in Different Hours After Retiring

Frequencies in per cent of total instances occurring for every hour after retiring. Based on all cases from the 4 observers.

Hours after retiring	*Conscious Cues*							
	1. Excretory	2. Digestive	3. Bodily state	4. Associative	5. Secondary criteria	6. Intervening consciousness	7. Attitudinal or automatic	Total number of cases
1st...........	0	0	.38	.38	0	.24	.10	21
2nd...........	.08	.06	.47	.17	.06	0	.17	36
3rd...........	.36	.15	.68	.27	.09	.21	.06	33
4th...........	.23	.10	.59	.23	.08	.08	.16	51
5th...........	.41	.26	.52	.30	.19	.33	0	27
6th...........	.29	.21	.50	.36	.14	.36	.07	14

every cue is reported for the successive hours after retiring. The frequencies are percentages of the total number of cases in which reports were made during the particular hour. Similar tables for the separate individuals do not give enough additional information to make their presentation worth while.

Table VII shows that the excretory and digestive cues do not become effective until the third hour after retiring. This relation holds just as positively for every one of the separate observers (excretory, *L, E,* and *R;* digestive, *L* and *E*) as it appears in the averages.

The cues from bodily state are, on the average, utilized least often in the first hour and most often in the third and fourth hours. Individually, however, the relation holds only for *L* and *E*. *B* seldom uses the cue. *R* uses it very frequently indeed, but indifferently at all times. The combined function of *L*'s two bodily cues approximates the average. *E* exaggerates the condition of the average; his frequencies for successive hours are 0, 43, 91, 82, 60, and 67%.

E's 'associative' cues, as we should expect, are used most frequently at the two extremes, *viz.* in the first and in the

fifth and sixth hours. The frequencies are 80, 57, 55, 64, 80, and 83% for the successive hours. *R*'s 'clear consciousness' is most frequent as a cue in the first hour, and infrequent thereafter. As *L* and *B* do not report this cue, the figures of Table VII are a combination of the functions for *E* and *R*.

The increase of secondary cues toward the end of the nocturnal period may be explained by the increased wakefulness of the observers, especially *B*, at this time. Thus it might be that, while lying awake just before the ringing of the alarm, the passing automobile, or crowing rooster, or striking clock would be noted by the observer. It is this same wakefulness, together with the increased frequency of dreaming toward morning, that introduces cues from the occurrence of intervening consciousnesses. The function is not really recurrent, however, as the figures of the table would seem to show. The high initial value is entirely due to *B*, who was especially wakeful during the first hour after retiring, but used these cues very little thereafter. The increasing final values come from the reports of *E*, whose frequencies for successive hours are 0, 0, 36, 27, 40, and 50%. *L* and *R* do not contribute materially to the average.

There is no reason to assume that the attitudinal or automatic judgments are dependent upon the time at which they are made.

We have been employing in this paper a method of *informational* report. The observer has been asked, not to describe mental process, but to report meanings, to designate cues, to make a judgment and state what data mean it or are relevant to it. It might be permissible to question the validity of such a verbal designation of cues and to doubt the adequacy of a method of report to the problem in hand. We have no desire to enter at this time into a full discussion of this methodological issue. We would contend, however, that our results do, in a measure, justify themselves. That there are cues, the small size of the objective errors attests. That these cues are conscious, we may perhaps not be assured until we have observed that the same cue, within certain limits of variation, leads regularly to the same judgment. By noting a constant concomitance between datum and judgment we

might, for example, demonstrate empirically a relevance of the datum to the judgment; but we have allowed for no such demonstration in these experiments. Instead we have asked the observer himself to designate relevance. Now a test of the adequacy of such designations of relevance would be to re-establish these data and have the observer state again what they indicated. Such an experiment we tried with *E*. *E* was shown his reports several months after the experiment without the record of the actual time or of his previous guess, and was asked again to estimate time on the basis of his written report. His constant error in this retrial was —15 minutes as against +28 minutes in the original experiment. His weighted average error in the retrial was 40% as against 45% in the original experiment. The improvement was due, undoubtedly, to the fact that he had spent much time in the interval in working over the data of this experiment and that he was thus able to profit by a knowledge of his previous mistakes. His ability to make as accurate judgments on the basis of the recorded informational statements as he did under the original conditions shows, however, that these statements really are cues of sufficient precision to account for the estimations which were actually made. Hence we may conclude that there is no evidence that an observer can not be set to report by informational statements the cues relevant to a given judgment; and, furthermore, that these cues are conscious, in the sense that they are adequate as reported.

Conclusion

Our observers, on being wakened from sleep, proved able to estimate time with a degree of accuracy which was approximately one-half the accuracy that chance guesses would have given. Every observer showed a constant tendency to make his estimation too late. There was no general tendency for the observers to improve with practice, although they were made aware of the direction and amount of their errors as the experiment progressed. One observer, however, was able to decrease his error slightly when subsequently he reinterpreted his recorded cues in the light of knowledge of his particular tendencies to error.

In general, the errors tended to be greatest at the time of deepest sleep. For three observers (*L, E, B*) this relation meant that the errors were minimal in the first hour after retiring, maximal about the third hour, and of intermediate magnitude in the fifth and sixth hours. One observer (*R*), who seems to have dropped at once into sound sleep, made maximal errors in the first hour and increased in accuracy thereafter.

We find that the conditions of these temporal judgments were conscious in the sense that observers were able to make verbal, informational reports of cues relevant to the meaning of time. The adequacy of such an account to the temporal meaning was attested by the ability of one observer, after the experiment, to reinterpret into judgments of time his recorded statements with no greater error than he had made in the original trials.

The most frequent and useful cues were those which depended upon the general bodily state. They were of two kinds: the cues of fatigue-and-restedness (*L*), which indicated directly the duration of sleep; and the cues of inertness-and-restlessness (*L, E*) or degree of sleepiness (*B, R*) which indicated directly the depth of sleep, and indirectly the duration of sleep, according to the observer's assumption of the time of deepest sleep. The correct interpretation of this cue depended on the type of the observer; light sleep had a different significance for those subjects who went to sleep gradually (*L, E, B*) than for the one who dropped into deep sleep almost at once (*R*).

For two observers (*E, R*) the cues from bodily state were supplemented by associative ones. For *E* these cues were especially effective at the beginning and end of the sleeping period, where the cue from bodily state was equivocal. In the early part of the period they consisted of facile logical continuity of the conscious ideas with the topic in mind at the time of retiring; at the end of this period the failure of this continuity served as a cue.

The course of the excretory and digestive functions furnished a somewhat less accurate basis for the judgment of time. After the third hour of sleep, 'bladder-sensations'

(*L, E, R*) and 'indigestion-sensations' (*L, E*) became increasingly intense and constituted a rough indicator of lateness, which served as a check upon other cues.

A memory of periods of wakefulness or of dreaming was generally taken as indicating a late hour. Dreaming at the time of waking was not consciously adopted as an index of light sleep.

All observers found the designation of cues difficult, and failed at times in the task. Occasionally they reported that the judgment had been 'attitudinal' or 'automatic.' As we have pointed out, there seems to be no reason, however, for supposing that these judgments are to any considerable extent determined unconsciously, in the sense that they depend on unreportable cues.

In only a few cases did external criteria appear to be operative. Accordingly, we may conclude that the course of conditions within the individual organism during sleep is capable of giving rise to conscious temporal cues, adequate to the meaning of time within the limits of error set forth above.

THE SELECTIVENESS OF THE ACHROMATIC RESPONSE OF THE EYE TO WAVE-LENGTH AND ITS CHANGE WITH CHANGE OF INTENSITY OF LIGHT

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In a previous paper, "Radiometric Apparatus for Use in Psychological and Physiological Optics—Including a Discussion of the Various Types of Instruments that have been used for Measuring Light Intensities," *Psychological Review Monographs,* XXIV, 1917, we have pointed out that the selenium cell, the photographic plate, the eye and the photo-electric cell are all selective in their response to wave-length and that in case of the first three the amount of this selectiveness varies with the intensity of the light used.[1] It was also stated that one of the purposes to which the apparatus described in that paper is to be devoted, is a study of the selectiveness of the eye's responses to wave-length and to intensity. Bearing on this point we need scarcely repeat that the kind and amount of selectiveness shown by the eye in its responses can be determined only by comparing it with an instrument whose responses are non-selective or directly proportional to the energy or physical value of the light waves employed. The instrument we have selected and described as most feasible for this purpose in the present stage of development of radiometric apparatus is the thermopile.[2]

Using the responses of the thermopile, therefore, as a

[1] With regard to whether or not the selectiveness of response of the photo-electric cell changes with the intensity of the incident light, investigators disagree. In the article referred to above will be found a résumé of the literature on this point.

[2] For a similar investigation by means of the thermopile of the selectiveness of action of the selenium cell, see A. H. Pfund, *Philosophical Magazine,* vii., (6) 1904, p. 26; see also Brown and Sieg, *Physical Review,* iv., 1914, p. 48.

It is obvious that the deviations from proportionality to the energy

standard of reference we have undertaken a somewhat extensive investigation of the selectiveness of both the achromatic and chromatic responses of the eye to wave-length. Preliminary to the detailed report of this investigation which cannot be made for some time, it may not be out of place to give here a few comparisons of the achromatic responses of the eye with those of the thermopile. These comparisons were made four years ago and have been withheld from print in the hope of a speedier completion of a larger part of the study. They have in general taken three forms: (a) a comparison at different intensities of the photometric value of stimuli made equal in energy value; (b) a similar comparison of stimuli having the relative energy values of a prismatic spectrum of a given type; and (c) a comparison at different intensities of the energy values of stimuli made equal photometrically. This, it scarcely need be pointed out, is only part of the programme that should be followed in making a study of the peculiarities and characteristics of the achromatic response to wave-length and in carrying out the interchecking of methods that is needed for making such a study; but as yet time has not been available to cover this broader field even in a preliminary way.

In this preliminary work time has been had for the use of only a limited number of stimuli and intensities. In later work results will be given for a part of the work, at least, for a greater number of points in the spectrum and for a greater number and range of intensities of light. The lights used as stimuli were a red, orange, yellow, yellow-green, green, green-

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of the light waves which are shown in the responses of the selenium cell, the photoelectric cell and the photographic plate are in need of investigation as well as those which are shown in the responses of the eye. Until these deviations are known quantitatively and corrected for in each individual apparatus, instruments of the type can not be used directly to measure energy. They may be used to advantage, perhaps, as balancing or equating instruments, but then only when the lights employed are of the same composition; or again in cases where it is possible and feasible to determine and use correction factors. If the lights are not of the same composition it is obvious that the selectiveness of response of the instrument will make the balance a false one or one not proportional to the actual amounts of energy involved.

blue, blue, violet and a mixed or white light. The colored lights unless otherwise specified were narrow bands taken from the following regions of the spectrum: $655\mu\mu$, $616\mu\mu$, $580\mu\mu$, $553\mu\mu$, $522\mu\mu$, $488\mu\mu$, $463\mu\mu$, and $439\mu\mu$. In order to get the high intensities needed at the photometer head, the analyzing slit was purposely made somewhat wide,—0.5575 mm. This width of slit was kept constant for all parts of the spectrum. If our problem had been such that smallness of range of wave-length had been a more important condition than intensity, a narrower slit would have been used. We have not considered, however, that smallness of range of wave-length was as important a condition to secure for the purposes of this investigation as the range of intensities that was made possible by using the greater width of slit. The white light was obtained by synthesizing the spectrum between wave-lengths 730 and $432\mu\mu$. The above lights were used in making this comparison in part because of the importance to current work in heterochromatic photometry of a comparative knowledge of the eye's peculiarities of response to these lights; and in part because they are taken from those parts of the spectrum to which the eye shows its most significant changes in selectiveness of response with changes of light intensity. The lights, both colored and white, were taken from the spectrum (a) in order that the former should be as homogeneous as to wave-length as was practicable; and (b) in order that both should be free from the infra-red and ultra-violet radiations which would affect the thermopile but not the eye. In synthesizing the white light from the spectrum care was taken, therefore, to use only wave-lengths safely within the red at one end and the violet at the other.[2a] The light was examined in every case at the analyzing slit for impurities by means of a small Hilger direct vision spectroscope provided with an illuminated scale. When found, impurities were absorbed out by thin gelatines selected so as to cut out as little of the useful light as possible. These gelatines were placed over the analyzing slit and were held in position by short clips fastened to the front surface of the jaws, the edges of which formed the slit. The importance of securing a high

[2a] The work with the white light will be given in a later paper.

degree of purity when correlations are to be made between the responses of the eye for a given region of the spectrum and energy values is obvious, *i. e.,* in proportion to their energy the alien wave-lengths would affect the eye differently from the wave-lengths under investigation.[3]

In making this comparison between the relative photometric and radiometric values for the different wave-lengths for the first type of investigation mentioned above, it was desirable that the same amount and as nearly as possible the same radiation density of light should fall on the receiving surface of both instruments. This was accomplished by making both determinations at the same place and having the same cross section of the collimated beam of light fall on the receiving surface of the two instruments. In detail the procedure was as follows: The colored light was obtained by means of the spectroscopic apparatus described in a previous paper.[4] To prevent an undue reduction by spreading and to make possible the requirements mentioned above with regard to the incidence of equal amounts and densities of light on the receiving surfaces of the two instruments, the waves of light emerging from the analyzing slit of this apparatus were rendered approximately parallel by being passed through a collimating lens placed at a distance from the slit equal to its focal length. At a given place in the beam were mounted interchangeably the thermopile and the photometer head.[5] The thermopile was of the surface type[6] with a receiving area of 15 x 15 mm.

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[3] In their work on the determination of the visibility of radiation in the red end of the visible spectrum, Hyde and Forsythe (*Astrophysical Journal,* xliv, 1915, p. 289) found impurities to the value of about 20 per cent. at $\lambda = 0.76 u$. In our own work determinations made with and without precautions for absorbing the scattered light show differences in result which are great enough to be considered of significance. In the work of Nutting and of Ives on the visibility of radiation, nothing is said about stray light or about precautions for eliminating it or correcting for it.

[4] C. E. Ferree and G. Rand. A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral. *Journal of Experimental Psychology,* I., 1916, pp. 247-283.

[5] Owing to the smaller amount of energies represented in the shorter

[6] C. E. Ferree and G. Rand. Radiometric Apparatus for Use in Psychological and Physiological Optics. *Psychological Review Monographs,* xxiv., 1917.

The photometer head consisted of a 60° brass prism (a modified form of the type of photometer head known as the Ritchie wedge) of such dimensions that the two surfaces adjacent to the 60° angle were each equal in area to the receiving surface of the thermopile. These two surfaces were covered with magnesium oxide deposited from the burning metal. They received the standard and comparison lights and served as fields for the photometric comparison. In order that the same cross section of light should fall on the photometric surface as fell on the receiving surface of the thermopile, the arrangement of the apparatus was such that the photometer surface was normal to the beam of light. This relation of axis of beam of light to photometer surface was made to apply alike both to the standard and comparison lights. To compensate for possible inequalities in the coefficients of reflection of the two surfaces the two faces could be interchanged by rotating the prism 180° about its horizontal axis. In making the photometric determination the colored light was balanced against the light from a standardized lamp. The standard used was a single loop street series tungsten seasoned and standardized by the New York Electrical Testing Laboratories to operate at 10.7 candle-power. Further to secure constancy of light flux this lamp was operated by storage batteries. In making the photometric-radiometric comparison the colored lights were, as stated above, first made radiometrically equal to the violet at the point of work. The thermopile was then removed, the photometer head put in its place,

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wave-lengths, for example, in the blue and violet, it was a matter of some difficulty to make an energy measurement of these wave-lengths with a satisfactory degree of precision at the position of the photometer head. It was thought better, therefore, to determine a reduction factor representing the relation of the total amount of energy emerging from the slit and the amount that was contained in the cross section falling on the photometer head. This was done, for example, by measuring the red at the slit with the linear pile and again at the photometer head with the surface pile, the latter receiving precisely the same cross section as fell on the photometer head. This factor having been determined, the blue light was measured at the slit with the linear pile and the factor was applied to give the energy value of the cross section incident on the photometer head.

and the lights incident on the two surfaces of the prism brought to a photometric balance by adjusting the position of the standard lamp.

Since the procedure is somewhat unfamiliar to psychologists, it may not be out of place to give here a brief description of how energy measurements are made by means of a thermopile. A description of the procedure at one of the places at which measurements were made, namely, the analyzing slit will be sufficient to show in a general way the method we have used in making these measurements. The thermopile to be used is placed in position immediately behind the slit and a blackened aluminum shutter is interposed in the path of the beam of light between the slit and the end of the objective tube of the spectroscope. Preliminary to the exposure of the thermopile to the light to be measured, the current sensitivity of the galvanometer is tested by means of a special device provided for this purpose in the construction of the galvanometer.[7] With regard to this procedure it need scarcely be pointed out that the current sensitivity of the galvanometer varies with the period or time of the single swing of its needle system. Since it is not possible to control the field so as to get this period always the same, it is necessary, if results are to be compared, to take some sensitivity as standard and to convert all readings into deflections for the standard sensitivity by means of a correction factor determined at each setting. For a detailed description of the method of determining this factor, see *Psychological Review Monographs,* XXIV, 1917.

The thermopile is next connected with the galvanometer and the light allowed to fall on its receiving surface until a temperature equilibrium is reached (*ca.* three seconds). The deflections are read by means of the telescope and scale and the readings are corrected to standard sensitivity by means of the factor previously determined. The final step in the process of measuring is the calibration of the apparatus, *i. e.,*

[7] This device consists of a special galvanometer coil, dry battery circuit and switch board with finely graduated resistance. For a description of this device, see *Psychological Review Monographs,* xxiv., 1917.

the value of 1 mm. of deflection in radiometric units is determined for the area of thermopile exposed. To do this a radiation standard, the value of the radiations from which is already known, has to be employed. The standard used by us is a carbon lamp especially seasoned and prepared for the purpose by W. W. Coblentz[8] of the radiometric division of the Bureau of Standards. This lamp is placed on a photometer bar 2 meters from the thermopile and operated at one of the intensities for which the calibration was made, in our case 0.40 ampere. The thermopile is exposed to its radiations with the same area of receiving surface as was used in case of the lights measured, and the galvanometer deflection is recorded. From the deflections obtained the value of 1 mm. of deflection, or the radiation sensitivity of the apparatus under the conditions given, is computed from the known amount of energy falling on the surface of the thermopile. Having the factor expressing the radiation sensitivity of the apparatus, the deflections produced by the wave-lengths of light measured are readily converted into energy units. The radiation sensitivity of the linear thermopile used by us was computed in a given case, for example, from the following data. The energy value of the radiations per sq. mm. at a distance of 2 m. from the standard lamp operated by 0.40 amperes is 90.70×10^{-8} watts. The deflections of the galvanometer corrected to a sensitivity of $i = 1 \times 10^{-10}$ ampere produced by this intensity of radiation falling on the same area of receiving surface as was used in measuring the lights employed as stimuli, and corrected for the absorption of the glass cover of the thermopile, was 625.625 mm. The area of surface exposed was 6.862 sq. mm., and the time of exposure was 3 sec. The sensitivity of the instrument per sq. mm. of receiving surface was, therefore, 145×10^{-11} watts. By means of this factor the galvanometer readings produced by the different wave-lengths of light may be readily converted into the energy value of light falling on the receiving surface of the thermopile.

However, as previously stated, the comparisons treated of

[8] W. W. Coblentz. Measurements on Standards of Radiation in Absolute Value. *Bulletin Bureau of Standards,* xi., 1914, pp. 87-100.

in this paper were made four years ago. At that time we were not provided with a radiation standard for the calibration of our apparatus. Without this calibration, the stimuli employed could be equalized in energy or their energy values could be compared from the galvanometer deflections produced, but the amounts of energy used could not be expressed in the conventional units. If an expression of the intensity of the stimuli were to be made in energy units, all the work done at that time would have had to be repeated. There has not been time to do this in season for the present paper. All of the radiometric values needed for the comparisons made in this paper will be given, therefore, in terms of relative galvanometer deflections converted for convenience of representation into an arbitrary scale in which the largest deflection is given the value of 100. This scale has been constructed separately for each table in which relative energy values are represented.

TABLE I

Showing the change in the relative selectiveness of the achromatic response of the eye to wave-length produced by varying the intensity of the light. In this table is given the photometric value in meter-candles of wave-lengths selected from different parts of equal energy spectra sustaining to each other the following ratios of intensity: A, 1/2 A, 1/4 A and 1/12 A.

Stimulus	Photometric value of stimulus in meter-candles			
	Intensity A	Intensity 1/2 A	Intensity 1/4 A	Intensity 1/12
Red (655 $\mu\mu$)	0.98	0.47	0.15	0.015
Orange (616 $\mu\mu$)	1.345	0.66	0.35	0.103
Yellow (580 $\mu\mu$)	3.4	1.5	0.825	0.35
Yellow-green (553 $\mu\mu$)	4.02	1.92	0.96	0.777
Green (522 $\mu\mu$)	2.44	1.34	0.82	0.56
Green-blue (488 $\mu\mu$)	1.42	1.06	0.79	0.523
Blue (463 $\mu\mu$)	0.817	0.546	0.28	0.25
Violet (439 $\mu\mu$)	0.56	0.26	0.164	0.128

The results for the first type of comparison are given in Tables I and II and Chart I. In Table I, Column 2, are given the photometric values of stimuli used in making the investigation, all made equal in energy value to the violet (439$\mu\mu$), width of analyzing slit 0.5575 mm., corrected for impurities, in a prismatic (CS_2) spectrum given by a Nernst filament operated by 0.6 ampere of current. This will be called Intensity A. In Columns 3, 4 and 5 are given the results of a comparison of the photometric values of these same groups of wave-lengths made equal in energy at three lower intensities: 1/2 A, 1/4 A and 1/12 A.

Spectra 1/2 A, 1/4 A and 1/12 A were obtained from A by the use of the sectored disc with a proper ratio of open to

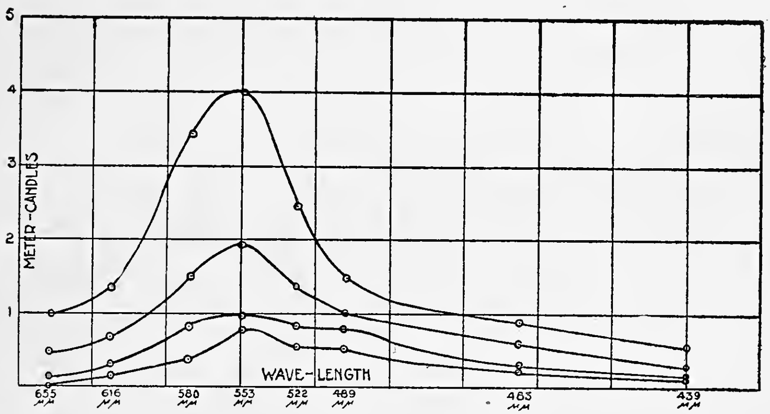
TABLE II

Showing the change in the selectiveness of the achromatic response of the eye to wave-length produced by varying the intensity of light. In this table are shown in per cent of the original value the photometric values of each of the colored lights when their energy values have been reduced respectively to 1/2, 1/4, and 1/12 of the values present in equal energy Spectrum A. The changes in the deviations of the relative photometric from the relative radiometric values produced by the changes of intensity may be noted by comparing the percentages in Column 2 with 50 per cent; in Column 3 with 25 per cent; and in Column 4 with 8.33 per cent.

Stimulus	Relation of photometric value of 1/2 A to A expressed in per cent	Relation of photometric value of 1/4 A to A expressed in per cent	Relation of photometric value of 1/12 A to A expressed in per cent
Red............ (655 $\mu\mu$)	47.96	15.31	1.53
Orange......... (616 $\mu\mu$)	49.07	26.02	7.66
Yellow......... (580 $\mu\mu$)	44.12	24.26	10.29
Yellow-green.... (533 $\mu\mu$)	47.76	23.88	19.33
Green.......... (522 $\mu\mu$)	54.92	33.61	22.95
Green-blue...... (488 $\mu\mu$)	74.65	55.63	36.83
Blue............ (463 $\mu\mu$)	66.83	34.27	30.60
Violet.......... (439 $\mu\mu$)	46.43	29.29	22.86

CHART I

Showing the change in the relative selectiveness of the achromatic response of the eye to wave-length produced by varying the intensity of the light. In this chart is represented the photometric value in meter-candles of wave-lengths selected from eight different parts of equal energy spectra sustaining to each other the following ratios of intensity: A, ½ A, ¼ A, and 1/12 A.



closed sector inserted between the collimator lens and the prism of the spectroscope. The photometric-radiometric comparison was made with the apparatus and by the method already described. A graphic representation of these results is given in Chart I.

In order to show the changes in the selectiveness of the achromatic response to wave-length produced by changing the intensity of light, Table II has been prepared. In the several columns of this table are shown in per cent. of the original value the photometric values of each of the colored lights when their energy values have been reduced respectively to 1/2, 1/4 and 1/12 of the values present in Spectrum A. Since A is an equal energy spectrum, the reduced spectra are also equal energy spectra. The changes in the deviations of the relative photometric from the relative radiometric values produced by the changes of intensity will be noted by comparing the percentages in column 2 with 1/2 or 50 per cent.; in column 3 with 1/4 or 25 per cent.; and in column 4 with 1/12 or 8.33 per cent. In this table a high value of the percentage

expressing the relation of the photometric values of A, 1/2 A, 1/4 A and 1/12 A indicates a relatively slow change in photometric value for a given change of intensity; and a low per cent. value indicates a relatively rapid change. Column 2 shows that when the equal energy spectrum A is reduced one-half in intensity, green ($\lambda 522 \mu\mu$), green-blue ($\lambda 488 \mu\mu$) and blue ($\lambda 463 \mu\mu$) darken relatively slowly; and yellow ($\lambda 580 \mu\mu$) the most rapidly. Columns 3 and 4 show that for reductions to one-fourth and one-twelfth, the slowest rate of darkening is still from $522 \mu\mu$-$463 \mu\mu$; the most rapid rate, however, has shifted to the red ($\lambda 655 \mu\mu$).

The results of the second type of comparison are given in Tables III and IV and in Chart II. In Table III are given the results of a comparison of the photometric values of the wave-lengths used in the preceding determinations from spec-

TABLE III

Showing the change in the achromatic response of the eye to wave-length produced by varying the intensity of light. In this table is given the photometric value in meter-candles of wave-lengths selected from different parts of prismatic spectra not equalized in energy, sustaining to each other the following ratios of intensity: A, 1/2 A, 1/12 A, and 1/45 A. The source of light was a Nernst filament operated by 0.6 ampere of current.

Stimulus	Photometric value of stimuli in meter-candles				Relative energy value of stimulus for Intensity A at photometer head
	Intensity A	Intensity 1/2 A	Intensity 1/12 A	Intensity 1/45 A	
Red....... (655 $\mu\mu$)	2.86	1.18	0.405	0.128	100.00
Orange.... (616 $\mu\mu$)	6.96	2.85	0.70	0.34	58.74
Yellow.... (580 $\mu\mu$)	7.74	3.34	0.92	0.458	30.36
Yellow-green (553 $\mu\mu$)	7.12	3.1	0.78	0.42	19.78
Green..... (522 $\mu\mu$)	5.50	2.76	0.55	0.30	13.59
Green-blue. (488 $\mu\mu$)	3.60	2.25	0.49	0.25	8.39
Blue...... (463 $\mu\mu$)	0.80	0.535	0.242	0.12	6.34
Violet..... (439 $\mu\mu$)	0.56	0.26	0.128	0.07	6.16

TABLE IV

Showing the change in the achromatic response of the eye to wave-length produced by varying the intensity of light. In this table are shown in per cent of the original value, the photometric values of each of the colored lights when their energy values have been reduced to 1/2, 1/12, and 1/45 of the values present in prismatic Spectrum A. The changes in the deviations of the relative photometric from the relative radiometric values produced by the change of intensity may be noted by comparing the percentages in Column 2 with 50 per cent; in Column 3 with 8.33 per cent; and in Column 4 with 2.22 per cent.

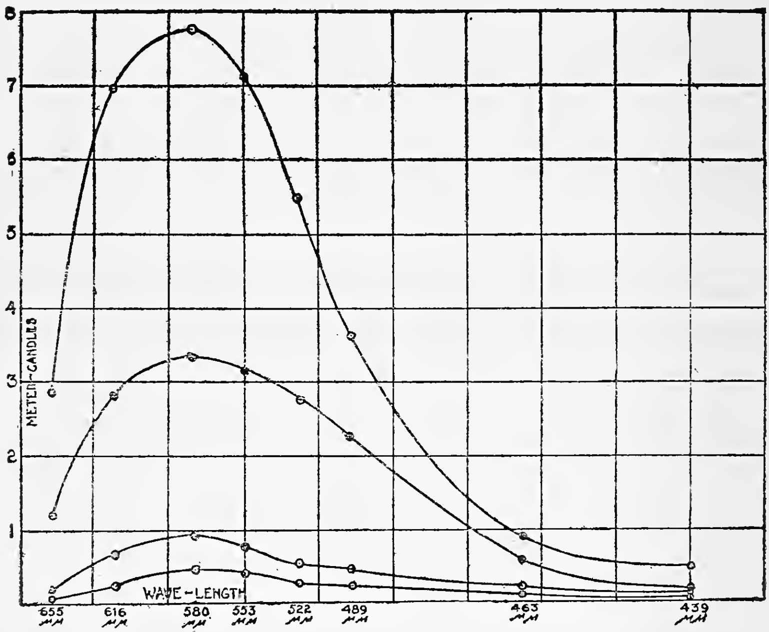
Stimulus	Relation of photometric value of 1/2 A to A expressed in per cent	Relation of photometric value of 1/12 A to A expressed in per cent	Relation of photometric value of 1/45 A to A expressed in per cent
Red (655 $\mu\mu$)	41.26	14.16	4.47
Orange (616 $\mu\mu$)	40.95	10.06	4.89
Yellow (580 $\mu\mu$)	43.15	11.89	5.92
Yellow-green (553 $\mu\mu$)	43.50	10.95	5.90
Green (522 $\mu\mu$)	50.20	10.00	5.50
Green-blue (488 $\mu\mu$)	62.50	13.61	6.94
Blue (463 $\mu\mu$)	66.88	30.25	15.00
Violet (439 $\mu\mu$)	46.43	22.86	12.50

tra of four intensities not equalized in energy: A, 1/2 A, 1/12 A, and 1/45 A. Intensity A is that given by the Nernst filament operated at 0.6 ampere of current. Intensities 1/2 A, 1/12 A, and 1/45 A were gotten by reducing Intensity A by means of a sectored disc. In making these comparisons there was, as is stated above, no attempt at an equalization of the energies of the lights employed. It was desired to find their relative photometric values at the four intensities with the type of distribution that occurs in such a spectrum as was used by us (prismatic CS_2 with gelatines interposed at the analyzing slit for the absorption of impurities) with the given width of analyzing slit.

This distribution is shown in Column 6, Table III and in Chart III.

Chart II

Showing the change in the achromatic response of the eye to wave-length produced by varying the intensity of light. In this chart is represented the photometric value in meter-candles of wave-lengths selected from nine different parts of prismatic spectra not equalized in energy, sustaining to each other the following ratios of intensity: A, ½ A, 1/12 A, and 1/45 A. The source of light was a Nernst filament operated by 0.6 ampere of current.

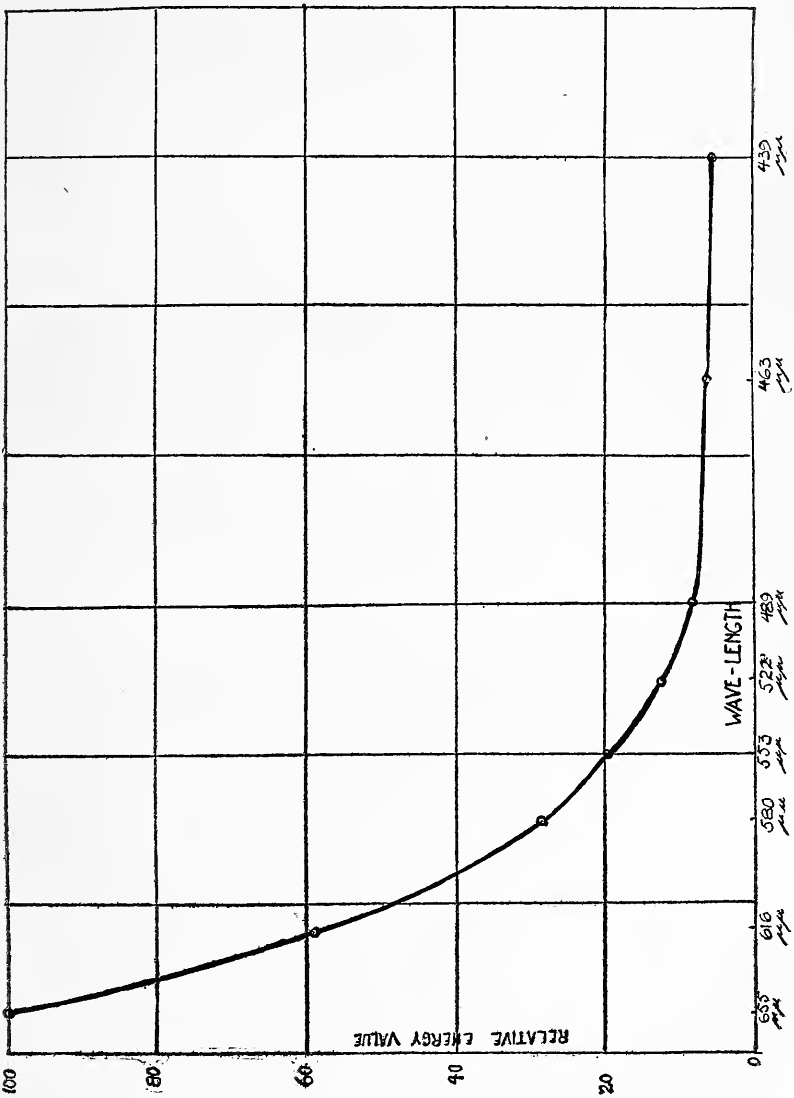


A graphic representation of the results of Table III is given in Chart II. Supplementary to Chart II, Chart III has been prepared. In this chart is given a graphic representation of the energy values of the colored lights used for Intensity A, that is, a graphic representation at eight points of the distribution of energy in the spectrum used by us when the width of the analyzing slit is equal to 0.5575 mm. This chart is constructed from the results in Column 6, Table III. In plotting the curve wave-lengths are represented along the abscissa and relative energy values along the ordinate.

Table IV has been prepared to make the same kind of

CHART III

Showing a graphic representation of the relative energy values of the colored lights used for Intensity A, Chart II and Tables III and IV.



showing of the results in Table III as was made in Table II for the determinations for the equal energy spectra. In this table are shown in per cent. of the original value the photometric values of each of the colored lights when their energy values have been reduced respectively to 1/2, 1/12, and 1/45 of their values at Intensity A. The changes in the deviations of the relative photometric from the relative radiometric values produced by the changes of intensity may be seen by comparing the percentages in Column 2 with 1/2 or 50 per cent.; in Column 3, with 1/12 or 8.33 per cent.; and in Column 4 with 1/45 or 2.22 per cent. In this table it is again seen that in general a rapid decrease of photometric value for a given decrease in energy is characteristic of the long wavelengths and a relatively slow decrease of the short wavelengths.

The foregoing results express relations between the photometric and radiometric values of the light waves employed at the given intensities. While this type of relation may be of interest to the physicist and more particularly to the lighting specialist, it does not permit of a rating either of amounts of response, or of sensitivity or power of giving response in a way which is, strictly speaking, quantitative. That is, in order that the rating or comparison of sensitivities may be made quantitative, it must be possible from the data at hand to compare numerically both the amounts of response and the amounts of stimulus used to give the response. In this connection it need scarcely be pointed out that the photometric values plotted in the preceding curves cannot be considered as amounts of response or sensation quantities, or as sustaining any simple relation to amounts of response. Two surfaces, for example, illuminated respectively by four and one meter-candles of light do not arouse sensations which sustain to each other the ratio of four to one, nor have we any knowledge of what ratio they do sustain to each other. That is, the photometric units: the candle, the lumen, the meter-candle, the lambert, etc., are all physical (not sensation) magnitudes differing in kind essentially from the erg, the watt, etc., only in that in photometric practice their comparison with the standard is made by the eye, the responses of which are not pro-

portional to the kinetic energy of the light waves. While, therefore, a determination of the photometric and the relative radiometric values of the wave-lengths shows that the eye is selective in its response to wave-length, the results are not obtained in a form that will permit of a simple numerical comparison. To have done this by a photometric method the responses should have been brought to equality and the stimuli used to give the equal responses should have been estimated radiometrically instead of the converse procedure which was used in the preceding work. For with equal responses empirically determined and ratings of stimuli that can be put in a numerical scale, an expression can be given to the relative sensitivities which is itself numerical.

That is, in connection with the problem of the quantitative rating of sensitivities, it is scarcely necessary to point out that without a means of rating the stimuli which treats all wave-lengths alike or which in other words gives values directly in terms of kinetic energy, an estimate cannot be made of relative sensitivities which can be considered as quantitative. In other words, with the introduction of radiometric treatment of the stimulus in the various laboratories the possibility of a comparison of retinal sensitivities that can be considered as quantitative to a degree that would be acceptable in the rating of a physical instrument, has been presented for the first time. In addition, however, it is obvious that an equally important point to be considered in connection with the problem of rating sensitivities quantitatively, is what amounts of response can be employed with sureness of principle for the purpose. The amounts of response that can be determined with an acceptable degree of precision are, it will be remembered, equal responses, the threshold liminal and differential, equal sense differences, and the mean or average deviation of a determination of any one of these. Of these quantities the last can obviously be used with the least *a priori* sureness of principle in a quantitative rating of sensitivities, if, as we have stated, it is necessary in making the rating quantitative that we be able to compare numerically the amounts of response employed. The thresholds liminal and differential could conform to this requirement only on the assumption that they represent equal amounts of

response of the sense-organ. There is, however, no way of proving such an assumption. If they are accepted as equal, it must be on the grounds of logical self-evidence. Many, however, are unwilling to grant their equality on these grounds. Equality of response and equal sense differences seem alone, therefore, to be surely capable of numerical comparison as sensation quantities, and of these two the determination of the former is much the more feasible experimentally and has the much wider range of applicability. Now in the use of a sense-organ as a measuring instrument where several methods are proposed differing in sureness of principle, precision, range of applicability, etc., as is the case here, it is customary to choose the one having the greatest *a priori* sureness of principle as a standard and to check up the others against it. If they give results which agree with it in the average their use is considered permissible. For example, in photometry the equality of brightness method is generally conceded to have the greatest *a priori* sureness of principle for the rating of lights for the use of the eye and is accepted, therefore, as a standard for this purpose in terms of which to evaluate other methods which may have advantages of precision or convenience of application in certain situations. The problem of the quantitative comparison of sensitivities seems to present an analogous case. The use of equal amounts of response seems to have the greatest sureness of principle; but it is not applicable to all cases in which a quantitative rating of sensitivities is desired. The use of the limen or just noticeable difference has, for example, a much broader applicability and its determination has perhaps an advantage in precision when a comparison is wanted between monochromatic stimuli differing in color value. Obviously, therefore, a comparative study should be made of the different possibilities of rating sensitivities in situations where all are applicable for the purpose of determining whether or not a reasonable degree of agreement obtains.[9] In our further work on the determination of the

[9] Because of the lack of a simple relation between the response and stimulus for a given wave-length or, more properly speaking, a small range of wave-lengths throughout the intensity scale, and of a similar relation for different wave-lengths at corresponding points in this scale,

achromatic sensitivity of the eye to wave-length, this will be made a prominent feature of the study. Achromatic sensitivity is selected for this purpose because it is a case for which all the determinations mentioned above may be made.

Obviously in the development of methods of working in new fields—and the quantitative rating of sensitivities is from the standpoint of its degree of development, at least in vision, a new field—counsel should be taken of work and methods already well established. A pattern for the rating of sensitivities of sense-organs may be had in the practice with regard to the physical recording instruments. In the rating of the sensitivities of two galvanometers it may be pointed out, for example, that the sensitivity to each is expressed for comparative purposes by the amount of current that is required to produce one unit of deflection. Such a treatment of the sense-organ as a recording instrument is not possible unless it be assumed that its responses can be laid off in equal divisions or units. However, the underlying quantitative principle that both the amounts of response and amounts of stimulus must be numerically comparable is satisfied by making the comparison on the basis of equal amounts of response. While convenient it is by no means necessary that the results be expressed in unit terms. In short, the best that can be done is probably to accept equal responses as having *a priori* the possibility of quantitative comparison and evaluate the possibilities of other means of rating being strictly quantitative in terms of their agreement with this method taken as a standard.

The work of rating sensitivities based on a correlation of equal sensation responses and the energy values required to produce these responses brings us to our third type of comparison, namely, a comparison of the energy values of stimuli made photometrically equal. As an example of this method of

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it is scarcely to be expected that a close agreement will be obtained in a rating of sensitivities made by the limen and just noticeable difference with that obtained when equal amounts of response are employed. However, a more definite knowledge is needed on the point before there can be any systematic treatment of the problem or a fair comparison and evaluation of results.

rating sensitivities Tables V-VIII and Charts IV-V have been prepared.

In carrying out this work the lights were made photometrically equal and their corresponding energy values were measured.[10] The stimuli were taken from a prismatic (CS_2)

TABLE V

Showing the change in the selectiveness of the achromatic response of the eye produced by varying the intensity of light. In this table are given the relative energy values of wave-lengths selected from seven different parts of spectra made photometrically equal at 75, 50, 25 and 12.5 meter-candles. The results of this and the following table represent also a determination of sensitivity by a method according to which both the amounts of response and the amounts of stimulus are numerically comparable. The comparative sensitivities for a given intensity should be proportional to the reciprocals of the relative energy values given in this table for that intensity.

Stimulus	Relative energy value of stimuli at pupil of eye			
	75 meter-candles	50 meter-candles	25 meter-candles	12.5 meter-candles
Red (660 $\mu\mu$)	88.23	44.50	15.34	6.14
Orange (619 $\mu\mu$)	15.37	8.14	3.39	1.58
Yellow (582 $\mu\mu$)	6.44	3.47	1.55	0.744
Yellow-green (560 $\mu\mu$)	5.18	2.85	1.29	0.647
Green (523 $\mu\mu$)	9.65	5.29	2.41	1.114
Green-blue (502 $\mu\mu$)	25.89	15.54	6.04	3.02
Blue (469 $\mu\mu$)	100.00	53.54	24.04	9.62

[10] The method of working here is similar to that used by Nutting (*Transactions Illumin. Engineering Society*, iv, 1914, pp. 633-643; also *Philosophical Magazine*, xxix, 1915, (6), pp. 301-309) in the determination of what he has called the visibility curve for the eye, with the following exceptions: (1) he used the method of flicker instead of the equality of brightness in making his photometric equalizations, i.e., a flicker balance instead of an equal sensation balance was obtained; and (2) compatible with his problem, namely, the determination of the visibility constants for a group eye, more especially the principal one, the maximum ratio of the candle to the watt, he used a greater number of observers and a much greater number of points in the spectrum. As a third point, it may also be mentioned that apparently he has used no precautions to obtain greater purity of light than is given by a single prism spectroscope. (See footnote, 3, p. 283.)

Visibility curves have been determined also by Thürmel (Das Lum-

spectrum of a Nernst filament operated at 0.7 ampere. They were narrow bands in the red ($660\mu\mu$), orange ($619\mu\mu$), yellow ($582\mu\mu$), yellow-green ($560\mu\mu$), green ($523\mu\mu$), blue-green ($502\mu\mu$), and blue ($469\mu\mu$). Four intensities of light were used, made equal respectively to 75, 50, 25 and 12.5 meter-candles, normal pupil. These higher intensities were selected because one of the objects of the investigation was to determine whether the selectiveness of the achromatic response to wave-length ceases at the higher intensities. Ives,[11] for example, determined his visibility curve at an illumination

mer-Pringsheimsche Spektral-Flickerphotometer als optische Pyrometer, *Annalen der Physik,* 1910, xxxiii, (4), p. 1139) and H. E. Ives (The Spectral Luminosity Curve of the Average Eye, *Philosophical Magazine,* xxiv., 1912, p. 853). Both of these men used the method of flicker but neither measured the energies of his lights directly. An attempt was made by both to get the energies of the lights employed by the use of the eye as an optical pyrometer. (On the use of the eye as an optical pyrometer, see Lummer and Pringsheim, *Jahresbericht d. Schles. Ges. f. vaterl. Kultur,* 1906; Beibl., 1907, p. 466). Throughout his work on the determination of the visibility curve Ives seems to have followed very closely the method used by Thürmel two years earlier.

While the methods that have been used for determining the visibility curve are similar in general principle, with the exceptions just noted, to that which we have outlined for a quantitative determination of sensitivities, it is not our understanding of that work that the lights were made photometrically equal for the reason that is given above, namely, that if sensitivities are to be determined in a way that permits of numerical comparison the amounts of response as well as the amounts of stimulus used in making the determinations must be numerically comparable. The reason that is assigned by Ives (*Philosophical Magazine,* xxiv, 1912, (6), p. 163), for example, is a technical one,—the photometric comparisons should all be made with the eye under the same illumination or in the same state of adaptation. In addition to this technical reason which is admittedly pertinent to the use of the eye in making any photometric balance between lights differing in color value, we have considered it of significance to call attention to this other reason which is of much more fundamental importance, we believe, to the quantitative determination of sensitivities for any purpose whatever, and which apparently has been overlooked.

[11] H. E. Ives. The Spectral Luminosity Curve of the Average Eye. *Philosophical Magazine,* xxiv, 1912, (6), pp. 853-863. The use of artificial pupil by Ives does not seem to be a matter of design, but a condition imposed upon the work by his apparatus. He says, p. 856: "In

TABLE VI

Showing the change in the selectiveness of the achromatic response of the eye to wave-length produced by varying the intensity of light. In this table are given the reciprocals of the scale values in Table V. The comparative sensitivity of the eye to the wave-lengths selected should be for a given intensity, directly proportional to the reciprocals of the scale values for that intensity.*

Stimulus	Reciprocals of scale values in Table VII			
	75-meter-candles	50 meter-candles	25-meter candles	12.5 meter-candles
Red (660 $\mu\mu$)	0.011334	0.02247	0.06519	0.16287
Orange (619 $\mu\mu$)	0.0651	0.12285	0.2950	0.6329
Yellow (582 $\mu\mu$)	0.15528	0.2882	0.6452	1.3441
Yellow-green (560 $\mu\mu$)	0.19305	0.3509	0.7752	1.5456
Green (523 $\mu\mu$)	0.10363	0.1890	0.41494	0.9009
Green-blue (502 $\mu\mu$)	0.0386	0.06435	0.16556	0.33113
Blue (469 $\mu\mu$)	0.01	0.01868	0.04160	0.10395

* The reader is cautioned against attempting from these data to compare sensitivities at different intensities. That is, while the amounts of stimulus are numerically comparable at the different intensities, obviously the amounts of response are not.

which he estimated to be about 25 meter-candles for his own eye, normal pupil (300 meter candles falling on a pupillary aperture of 1 sq. mm.), claiming that at this intensity the achro-

all the previous work an artificial pupil was used and the results were given in terms of meter-candles illumination as viewed through this 1 sq. mm. aperture [objective slit of spectroscope 0.5 x 2 mm.]. In working with a spectrometer the use of a small eye-slit is practically imperative. But in practical photometry an artificial pupil of this size would necessitate working at illuminations too high to be practicable with present illuminants if one had to attain the retinal illumination called for by the investigation here described. Were the pupils of all observers of the same size under the same conditions, a reduction factor might be obtained so that the luminosity curve could be found with the artificial pupil and used for a corresponding illumination with the natural pupil. Such, however, is not the case. In view of these facts it was considered advisable in the present research to have all curves made for a normal pupil illumination."

TABLE VII

Showing the change in the selectiveness of the achromatic response of the eye to wave-length produced by varying the intensity of light. In this table are shown in per cent of the original value, the energy value of the stimuli when their photometric values have been reduced from 75 to 50 meter-candles; from 75 to 25 meter-candles; and from 75 to 12.5 meter-candles. A high per cent energy value indicates that a relatively small decrease in energy is needed to produce the desired decrease in photometric value or that there is a relatively rapid darkening of the color with decrease of energy.

Stimulus	Per cent of energy value to original value when photometric value has been reduced from		
	75 to 50 meter-candles	75 to 25 meter-candles	75 to 12.5 meter-candles
Red (660 $\mu\mu$)	50.4	17.4	7.0
Orange (619 $\mu\mu$)	52.9	22.1	10.3
Yellow (582 $\mu\mu$)	53.8	24.0	11.5
Yellow-green (560 $\mu\mu$)	55.0	25.0	12.5
Green (523 $\mu\mu$)	55.8	25.0	11.5
Green-blue (502 $\mu\mu$)	60.0	23.3	11.7
Blue (469 $\mu\mu$)	53.5	24.2	9.6

matic response is practically, if not entirely, free from Purkinje effects. Nutting[12] for a similar reason used 350 meter-candles of light falling on a pupillary aperture of 1.465 sq. mm., contending that this intensity of illumination is "safely outside the range of the Purkinje effect."

These views, however, it will be remembered, are quite the opposite from those of Helmholtz and others of the earlier writers who believed that the eye changes its selectiveness of response to wave-length with change of intensity of light at the higher as well as at the lower intensities of light. This conclusion is drawn from a statement made by them that beginning with a spectrum of fully saturated colors and increasing the intensity of light, all the colors are found to tend

[12] P. G. Nutting. The Visibility of Radiation. *Philosophical Magazine,* xxix, 1915, (6), p. 303.

TABLE VIII

Showing the change in the selectiveness of the achromatic response of the eye to wave-length produced by varying the intensity of light. In this table the comparative sensitivities of the eye to the different stimuli at a given intensity are shown in a scale in which the highest sensitivity for that intensity is represented by 100. If there were no relative changes in the eye's sensitivity to wave-length with change of intensity of light for these high intensities, the values in this scale would be the same for each stimulus for the four intensities.

Stimulus	Relative sensitivity in a scale in which the highest sensitivity is represented by 100			
	75 meter-candles	50 meter-candles	25 meter-candles	12.5 meter-candles
Red (660 $\mu\mu$)	5.87	6.40	8.41	10.54
Orange (619 $\mu\mu$)	33.72	35.01	38.06	40.95
Yellow (582 $\mu\mu$)	80.435	82.14	83.23	86.32
Yellow-green (560 $\mu\mu$)	100.00	100.00	100.00	100.00
Green (523 $\mu\mu$)	52.18	53.87	53.53	58.29
Green-blue (502 $\mu\mu$)	19.99	18.34	21.38	24.74
Blue (469 $\mu\mu$)	5.18	5.32	5.36	6.73

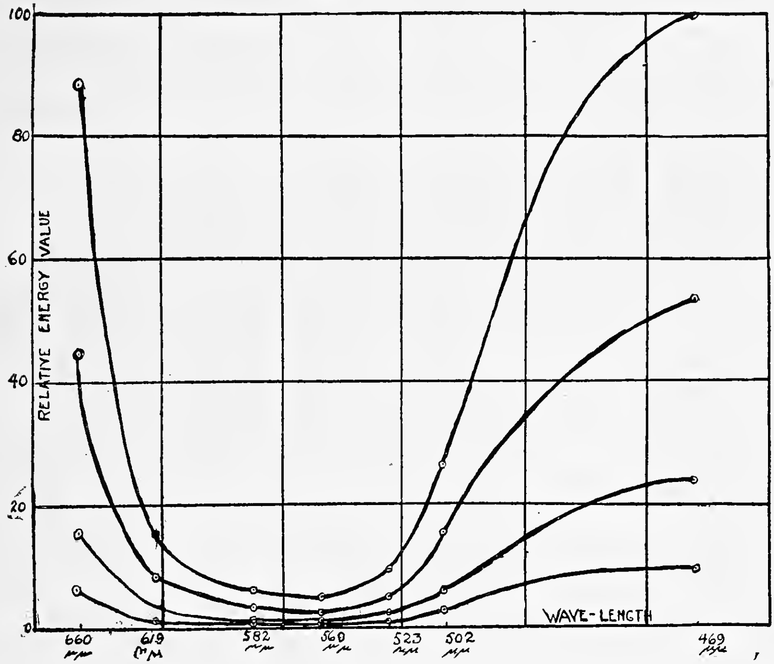
towards white and in so doing to change their luminosities at different rates.[13] (For example, see Helmholtz, *Poggendorff Ann. der Phys.*, 1852, *86*, p. 520; also *Handbuch der physiologischen Optik*, 1896, 2te Aufl., pp. 465-466; A. Chodin, *Sammlung phys. Abhandl. v. Preyer*, 1877, *1*, p. 33, ff., *E. Brücke, Sitzungsber. der Wiener Akad., Math.-Natur. Klasse*, 1878, *77*, (3), p. 63.)

Since in these as in all of the preceding determinations, a photometric value was wanted in terms of the power to arouse the achromatic sensation as the eye normally sees its brightness and not in terms of a flicker evaluation, the equality of brightness method was used in making the photometric balance. That is, the comparisons are based on an equality of

[13] In this connection it should be borne in mind that Nutting and Ives presumably refer to determinations made by the method of flicker, while the writers referred to above are considering the eye as it normally sees its brightnesses.

CHART IV

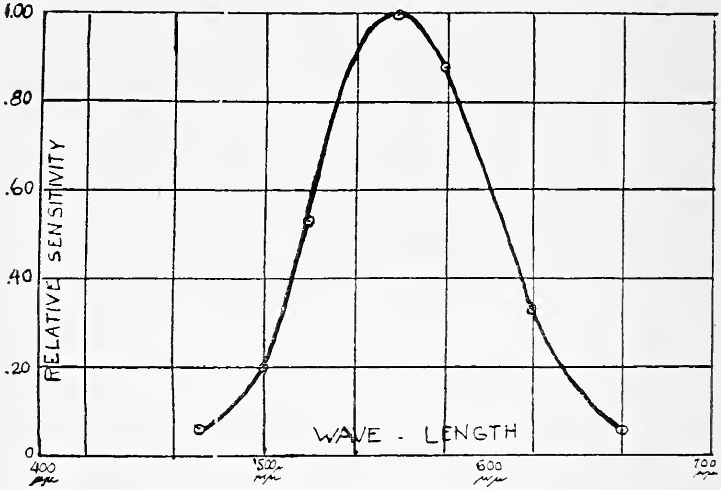
Showing the change in the selectiveness of the achromatic response of the eye produced by varying the intensity of light. In this chart are represented the relative energy values of wave-lengths selected from seven different parts of prismatic spectra made photometrically equal at 75, 50, 25 and 12.5 meter-candles.



brightness not a flicker balance. The plan of the apparatus used in making the photometric balance (spectroscope, photometric apparatus, etc.,) is indicated in Fig. I. The colored light was presented to the eye in the following manner. The eye-piece was removed from the spectroscope and a lens system was substituted consisting of two lenses L_1 and L_2, one to render the light emerging from the objective slit parallel and the other to focus it on the eye 30 cm. distant. By means of an extra set of jaws operating in the vertical, the length of the analyzing slit was reduced to a value which gave an image 3 x 1.49 mm. on the pupil of the observer's eye. This adjustment was maintained throughout the work. The size

Chart V

Showing a curve of achromatic sensitivity when the stimuli are made photometrically equal at 75 meter-candles.



of the photometric field was limited by a screen S, containing a stimulus opening 15 mm. in diameter. This screen was placed 20 cm. from the eye. Between this screen and the lens L_2 was inserted a small strip of metal, D, the inner edge of which was carefully beveled. When adjusted to the position used in making the photometric observation, this edge just bisected the photometric field. The surface of this strip was kept freshly coated with magnesium oxide deposited from the burning metal. This surface received the light from the standard lamp; the other half of the field was filled with light from the spectroscope. The spectroscope and lens system were shielded from the standard lamp by suitable screens. The photometric balance was obtained as follows. The standard lamp, a seasoned 32 cp. carbon lamp giving the color value of the carbon standard of 4.85 watts per spherical candle, was set at the position on the bar required to give the desired intensity of light in the photometric field, and the intensity of the colored light filling the other half of the field was varied until a brightness match was obtained. The changes in the intensity of the colored light required to give the match were

not made by varying the width of the collimator slit, as is often the case, because changes in the width of the collimator slit tend to give a variable purity of light,—a condition which would have given us more trouble in the selection of our filters to absorb stray light. Specially constructed sectored discs with a single open sector adjusted by a finely threaded micrometer screw and provided with a Vernier reading to minutes, were used instead for this purpose. That is, the collimator slit was set at a width which made the comparison field slightly brighter than the standard field for the group of wave-lengths in question and the gelatines required to absorb the alien wave-lengths were placed in position over the analyzing slit. These gelatines, as stated earlier in the paper, were held in place by short clips fastened on either side of the slit to the

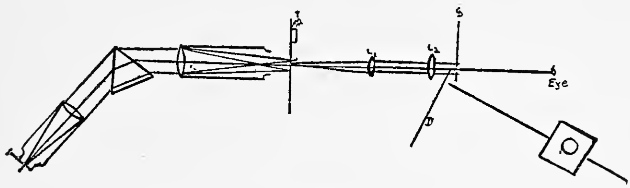


FIG. I

front surface of the jaws, the edges of which formed the slit. The width of the collimator slit and the gelatines were then kept constant, and the reductions needed to give the four intensities were made by means of the sectored discs, inserted between the analyzing slit and the lens L_1.

In Table V are given the relative energy values of the stimuli to give equal achromatic responses at the four photometric intensities used. These values are shown graphically in Chart IV. In constructing this chart as in case of all of the preceding charts, the wave-lengths are spaced to approximate the distribution in the prismatic spectrum. The comparative sensitivity of the eye to the groups of wave-lengths used should of course be as the reciprocals of the relative energy values required to give equal achromatic responses. The values of these reciprocals are given in Table VI. A graphic representation of the results for the highest intensity in this table is given in Chart V. In this chart for the sake of a closer

comparison with more recent work, the wave-lengths are given equal spacing along the abscissa and the scale employed (reciprocal at point of highest sensitivity = 1) is the same as was used by Nutting in his visibility curve.

In Table VII are shown in per cents of the values for 75 meter-candles the energy values of each of the group of wave-lengths when they have been made photometrically equal at 50, 25 and 12.5 meter-candles. In this table a high per cent. energy value indicates that a relatively small decrease in energy is needed to produce the desired decrease in photometric value; or expressed in other terms, indicates a relatively rapid darkening of the color with decrease of energy. The results show first of all, it will be noted, that change in selectiveness of response with change of intensity is still present in the region of the intensity scale included between 50 and 75 meter-candles, as well as in the regions included between 25 and 75 meter-candles and 12.5 and 75 meter-candles. A closer scrutiny shows further that in case of the reduction from 75 to 50 meter-candles the most rapid darkening with a decrease of energy occurs in the region of the blue-green. This effect is, it will be remembered, quite the opposite of that which was obtained at the lower intensities treated of earlier in the paper (see Table II, Columns 3 and 4). At these intensities the slowest darkening was obtained in the region of the blue-green and the most rapid in the region of the red. Moreover, for the reduction 75 to 12.5 meter-candles, the region of most rapid darkening shifts to the middle of the spectrum. In short, in passing from high to low intensities the region of most rapid change in selectiveness of achromatic response seems to shift from a region in the short wave-lengths at the high intensities, through the middle of the spectrum at the intermediate intensities, to the long wave-lengths at low intensities. In Table VIII the change in selectiveness with change in intensity is shown in still another way. In this table the comparative sensitivities of the eye to the different stimuli at a given intensity of light are represented in a scale in which the highest sensitivity for that intensity of light is arbitrarily given a value of 100. If there were no relative change in the eye's sensitivity to wave-length with change of

intensity of light, the values in this scale would be the same for the four intensities of light. A more detailed investigation of this point for a greater number and range of intensities and for a greater number of points in the spectrum will be carried out later.

In conclusion we would again point out that the foregoing results are presented as preliminary and illustrative of some of the ways in which the selectiveness of the achromatic response of the eye to wave-length and its change with change of intensity may be studied by the help of energy measurements, rather than as a finished investigation of any one point. The work is discursive rather than intensive and in this regard was actuated by an entirely different purpose from that, for example, which has prompted the determination of the visibility constants for a group eye for the purpose of obtaining the mechanical equivalent of light, in which case a number of observers and a much greater number of points in the spectrum have been used. Moreover, since a sensation balance as the eye normally sees its brightnesses was wanted for the different intensities of light used, and not a flicker balance, all subjective equalizations of light intensities were made by the equality of brightness method. This choice of methods we consider alone compatible with the purpose of such studies as are here outlined, even were all other points of dispute waived with regard to the selection of a photometric method for other purposes and problems which may be encountered in the handling of light intensities.

TACTUAL DISCRIMINATION AND SUSCEPTIBILITY TO THE MÜLLER-LYER ILLUSION, TESTED BY THE METHOD OF SINGLE STIMULATION*

By Josephine N. Curtis, Psychopathic Hospital, Boston

In a critical article on ethnological tests of sensation and perception, Titchener has suggested[1] that under field conditions "all or none" tests, applied in single trials to numerous individuals, may prove more practical, and may yield results more satisfactory, at least for anthropological purposes, than tests involving repeated application or fractional grading. In many respects the difficulties and the sources of error in experimentation upon savages are the same as those encountered in working with abnormal subjects. It seemed to us, therefore, that by imitating the simplicity of task, apparatus, and method necessary under field conditions, and by running parallel series of tests on normals and abnormals, we might at least get some indication of the practicability of the proposed method.

Subjects

Our investigation was carried out between January and April, 1917. The total number of subjects used for the experiment on tactual discrimination was 444; for that on susceptibility to the Müller-Lyer illusion, 627.[2] We have grouped our subjects as follows:

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* Being Contribution of the Massachusetts Commission on Mental Diseases, whole number 186 (1917.6). The previous contribution was by Professor R. M. Yerkes, entitled A Point Scale for the Measurement of Intelligence in Adolescent and Adult Individuals, No. 185 (1917.5).

[1] E. B. Titchener, On Ethnological Tests of Sensation and Perception, with Special Reference to Tests of Color Vision and Tactile Discrimination in the Reports of the Cambridge Anthropological Expedition to Torres Straits. *Proceedings of the American Philosophical Society*, LV, 1916, pp. 204-236.

[2] It will be seen that the total number of cases does not always tally with the number given in a table. The reason for this is that not all

I. Superior normal subjects (157): successful business men (21), wives of successful men (40), professional women (27), professional men (21), business women (16), students in college or graduate school, men (11), women (16), well educated women with no occupation (5).

II. Average normal subjects (74): nurses (16), stenographers (16), male hospital employees (9), house-maids (6), dressmakers (3), clerical workers (3), clerks (2), telephone operators (2), laborers (2), no occupation (2), school-boy, delivery-man, elevator boy, steam-fitter, milkman, grocer, lumberman, painter, teacher of small school, sewing teacher, special teacher, housewife, matron, each (1).[3]

III. Feeble-minded subjects (162): girls at the Massachusetts School for the Feeble-Minded at Waverley (107), patients at the Psychopathic Hospital (45), patients at the Danvers State Hospital (10). The patients at the Danvers State Hospital and one of those at the Psychopathic Hospital were not given psychological examinations to determine their mental age. The remainder range in mental age from 5.2 to 18+.

subjects were given both tests. For example, four subjects (three dementia praecox and one feeble-minded) refused to be touched with the aesthesiometer, and one blind subject (dementia praecox) could not be shown the illusion. We have thought it simpler to drop such cases from the tables than to introduce a column showing the number of subjects to whom a particular test was not given.

[3] It was thought advisable to divide the normal subjects into these two groups since the difference in social status between the patients at State hospitals and the normals in our superior group was so great that it might conceal any difference due to mental condition. The type of patient at the hospitals may be seen from the following list of occupations of subjects used in this investigation. Housewives (23), no occupation (14), housemaids (12), laborers (10), students at various schools (13), factory workers (5), clerks (4), teachers (3), teamsters (2), polishers (2), electrical assistants (2), coachmen (2), section foreman, auto salesman, book seller, steam fitter, switchman, bootblack, carpenter, "sprinkler work," engineer, fireman, grocer, blacksmith's helper, piano tuner, attendant, freight handler, reporter, business man, tinsmith, machinist, physician, elevator man, oiler, steward, waiter, baker, bartender, furniture sandpaperer, lawyer, sheet metal worker, letter carrier, cashier, painter, arc trimmer, conductor, chemical engineer, cook, paper roller, bookkeeper, nurse, weaver, each (1), to say nothing of the considerable number whose occupation was not ascertained.

IV. Subjects suffering from dementia praecox either at the Danvers State Hospital or at the Psychopathic Hospital (61). Seventeen of these had been given psychological examinations and ranged in mental age from 8.5 to 18+.

V. Subjects at either of the two hospitals suffering from alcoholic psychoses (34), including: chronic alcoholism with deterioration (11), alcoholic hallucinosis (8), unclassified alcoholic psychoses (4), delirium tremens (3), acute alcoholism (3), Korsakoff's psychosis (2), chronic alcoholism (paranoid) (1), alcoholic dementia (1), habitual drunkenness (1). The twelve of these who were tested ranged in mental age from 8.7 to 16.5.

VI. Subjects at either of the two hospitals suffering from syphilitic psychoses (21), including: general paresis (15), neuro-syphilis (5), tabo-paresis (1). Of these only one received a psychological examination and he graded at mental age 9.2.

VII. Subjects diagnosed as Psychopathic Personalities (20).[4] Nineteen of these cases were given psychological examinations, and they ranged in mental age from 11.0 to 17.0.

VIII. Subjects at either of the hospitals suffering from manic depressive insanity (12), including: manic (7), depressed (3), mixed (2). Two of these cases were given psychological examinations, and graded at 13 and at 14 years mental age.

IX. Patients at the Psychopathic Hospital who were diagnosed as having "no mental disease" (21). They ranged in mental age from 8 to 17. It seemed unfair to class this group of subjects as normal, because their mere presence at a psychopathic hospital indicates that their reaction to life is not normal, even if the cause can not be laid to mental condition.

X. Patients at the two hospitals who cannot be arranged in groups by diagnoses because of the few cases for each disease (60). As we have used the data obtained from these subjects in our discussion where the question was one of age

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[4] This diagnosis is made at the Psychopathic Hospital following Kraepelin, and covers cases whose defect is not intellectual. Such cases are diagnosed elsewhere as defective delinquents, constitutional inferiors, feebly inhibited, etc.

or sex and not one of diagnosis, it may be well to list them here: cases where the hospital staff disagreed as to diagnosis (17), arteriosclerotic psychosis (10), psychoneurosis (5), pre-senile psychosis (3), retarded (3), subnormal (3), hysteria (2), speech defect (2), epilepsy (2), arterio-sclerosis *plus* chronic alcoholism, arterio-sclerosis *plus* cardio-renal changes, post-apoplectic arterio-sclerosis, involutional melancholia, Addison's disease, psychopathic constitution, unclassified paranoid, symptomatic depression, insane epileptic, post-epileptic psychosis, neurasthenia, thyrogenic myxodema, senile dementia, each (1).

There are thirteen negroes among our subjects: feeble-minded (5), alcoholic psychoses (3), general paresis, psychoneurosis, hysteria, retarded, and "no mental disease" each (1).

Method

The Müller-Lyer illusion was drawn in the center of a sheet of white paper 15 cm. by 9 cm. as follows: width of all lines .05 cm., length of horizontal bounded by arrow heads 3.5 cm., length bounded by arrow feathers 4 cm., arrows formed by lines .5 cm. in length, drawn at an angle of 30° to the main line.

The apparatus for the tactual experiments was an aesthesiometer consisting of a fibre-board base one-half inch squaie in cross-section into which from below were set, to serve as stimulators, two conical points of hard rubber projecting ⅝ in., at a permanent distance of 4 cm. from tip-center to tip-center, and from above a heavy U-shaped wire projecting 3 in., upon which a rubber handle slid easily and noiselessly. The weight of the instrument, effective for simulation, was exactly 30 grams. The tips of the conical points above mentioned were given a spherical form by the following procedure. The cone was turned down till the smaller end had a diameter slightly less than 1 mm. This tip was then roughly shaped. A hollow hemisphere of 1 mm. diameter was now formed by placing a steel ball in solder and removing it when the solder hardened. The cone was then placed in a drill-press and its tip made hemispherical by rotation in flour

of emery in the hemispherical depression. The resulting diameters of the tips, measured by a micrometer caliper, were 1.05 and 1.08 mm.

The experimenter in most cases was the writer. Tests were given, however, to about 40 men (patients and attendants at the Psychopathic Hospital) by Dr. F. J. O'Brien; and the Müller-Lyer illusion was shown to 42 normal men and women by Mrs. J. W. M. Nash, an experimenter with college training; to 46 normal men and women by Mrs. E. B. Curtis, an experimenter with some experience; and to 88 subjects (patients and hospital employees) at the Danvers State Hospital for the Insane by Dr. Anna H. Kandib.

It was found advisable to try first the illusion and then the tactual test; for when the reverse order was used, subjects sometimes imagined that a practical joke or a painful experience was coming. A preliminary statement was made to the normal subjects, to the effect that the writer was collecting data as to the differences in replies given to two questions by normal and abnormal subjects, and the assurance added that they were considered as normal. The other subjects were usually given the tests at the end of the routine hospital psychological examination, so that they took the tests as a part of the regular examination. In most cases in which the abnormal subjects did not have the full psychological examination, they were so accustomed to tests that they took ours as a matter of course. No subject ever refused to answer the question about the illusion. An occasional psychotic patient was unwilling to be touched unless he could see his arm during the operation; in these cases the aesthesiometer-test could not be carried out.

The illusion was presented with the horizontal bounded by arrow heads to the subject's right. The form of question used was: "Are the two halves equal?" If the subject answered "No," he was asked "Which is the longer?" If the subject replied, as some 4 or 5 of our superior normals did, "Two halves are always equal," or if the subject failed to comprehend the meaning of the question, the experimenter said "I mean, is it the same distance from here (pointing to the tip of the extreme left arrow) to here (pointing to the

middle arrow) that it is from here (pointing again to the middle) to here (pointing to the right arrow)?" With an occasional low-grade patient it was necessary to add "Is this line (pointing left) just as long as this line (pointing right)?" The directions for the tactual experiment were as follows: "Now I want you to put your right arm out here on the desk (or table, or chair-arm), and rest it down comfortably; then I want you to shut your eyes. I am going to say 'Ready,' and then I am going to press down lightly on your arm. Afterwards I want you to tell me what you felt." The aesthesiometer was applied longitudinally to the volar surface of the right arm, with the lower point about 2.5 cm. above the upper carpal fold, and avoiding, as far as possible, cords and veins. The subject never knew before the test that any apparatus was to be used and practically no subject saw it, even after reporting. In about half of the first few cases done, the answers were so indefinite (as, for example, "little pricking") that it could not be said how many pressures the subject felt. The further request "Show me where" was therefore introduced. In response to this demand the subject usually pointed to the places on the arm where pressure had been felt. This report was then recorded as "one point," "two points," "line," "line across arm," "area," and "small area." When a subject touched his arm with one finger and held that finger on a definite spot steadily, a record of "one point" was made; if however, he moved the finger around over an area of perhaps twice the size of the end of his finger, a record of "small area" was made; if he moved his finger (or hand) over a comparatively large portion of his arm, a record of "area" was made; if he moved his finger in a definite line the record was "line" or "line across arm" according as he moved longitudinally or transversely; and if he pointed to two definite places on his arm, or placed two fingers in definite places, the record "two points" was made. On the whole these records give a satisfactory statement of the number of points felt, for they never disagree with the first reply of the subject when that reply is exact about number of pressures felt, and we can but suppose that if the inexact replies had been given better expression, there would be agreement there. A slight

misunderstanding may arise, however, from statements of "area," since it is not clear whether pointing to an area means that a broad pressure was felt, or that the location of a small point was uncertain. Occasionally after stimulation in the tactual test the subject seemed still to be waiting. In such a case the question was repeated; and then, if no answer came or if the subject said "I don't know" or "Nothing," he was asked "Did you feel me touch you?" In all but one or two cases the subject replied "Why yes, of course;" and he was then asked to point out where he had been touched, and the expression of judgment was recorded. This kind of response was given six times only. One difficulty with the instructions used is that subjects who are unaccustomed to psychological experiments, or who are not sufficiently intelligent to grasp the entire meaning of the directions, open their eyes at the "Ready" signal, apparently thinking that the test is over. This, of course, interrupts the experiment before the stimulus is applied and necessitates further explanation. With the abnormal and inferior subjects it was therefore found advisable to hold a paper over the part of the arm stimulated so that the subject could not see the aesthesiometer in case he did open his eyes. Four subjects (1 general paresis, 3 arterio-sclerosis) were unable to feel anything even after four attempts.

Results

We have divided our results for the tactual experiment into those who felt two points definitely, and those who did not feel two points. All our classes of "line" and "area" are thus counted as "not two points." Seven of our superior normal subjects and one average normal (3 male and 5 female) reported more than two points, and these we have grouped in the "not two" column, although if the difference be taken as one of discreteness it might be more reasonable to place them with the "two points." The results for the illusion are divided into those saying that the "right" (horizontal bounded by arrow heads), those saying that the "left" (horizontal bounded by arrow feathers) was the longer, and those who called the two horizontals "equal."

TABLE I

RESULTS OF TESTS OF TACTUAL SENSITIVITY AND SUSCEPTIBILITY TO THE MUELLER-LYER ILLUSION

Subjects Grouped by Diagnosis

	Aesthesiometer Test			Müller-Lyer Illusion			
	No. of cases	Percentage of judgments		No. of cases	Percentage of judgments		
		"not two"	"two"		"right"	"equal"	"left"
Superior normal	77	77	23	154	47	53	0
Average normal	49	69	31	72	63	36	1
No mental disease	17	100	0	20	95	5	0
Psychopathic personality	20	100	0	20	65	35	0
Manic-depressive	9	89	11	11	100	0	0
Alcoholic psychoses	24	100	0	32	75	25	0
Syphilitic psychoses	15	93	7	21	86	14	0
Dementia praecox	27	100	0	60	78	20	2
Feeble-minded	150	97	3	157	80	15	5
Subjects Grouped by Mental Age							
4.5—5.4	1	100	0	0	....	....	....
5.5—6.4	2	100	0	2	50	0	50
6.5—7.4	16	94	6	15	87	13	0
7.5—8.4	31	100	0	34	64	18	18
8.5—9.4	39	100	0	45	89	9	2
9.5-10.4	26	96	4	26	96	4	0
10.5-11.4	41	98	2	42	76	24	0
11.5-12.4	29	100	0	35	89	11	0
12.5 and up	56	96	4	58	78	22	0
Normals	126	74	26	226	52	48	0
Subjects Grouped by Sex							
Male	135	91	9	195	61	39	0
Female	309	91	9	432	74	24	2
Subjects Grouped by Chronological Age							
0-14	24	96	4	30	90	10	0
15-19	59	98	2	71	72	27	1
20-29	177	86	14	187	65	32	3
30-39	73	99	1	100	70	29	1
40-49	45	93	7	90	78	21	1
50-59	32	97	3	79	63	37	0
60-69	6	100	0	35	54	46	0
70-79	2	100	0	7	86	14	0
80-89	0	0	0	4	100	0	0

The results of the experiments are given in Table I. If we consider first the results of the tactual experiment, we find that the percentage of normal subjects who report "two" is much larger than the percentage of abnormal subjects giving that response. Unfortunately the average per cent of "two" judgments by the normals is so low (about 26%) that there is little chance for differences between the classes of abnormals to appear. In our choice of the distance used we were misled by laboratory findings on superior, psychologically trained adults to choose the 4 cm. distance. If the distance between our aesthesiometer points had been greater, doubtless a greater per cent of abnormals would have given the judgment "two" and we might have found decided differences between, for example, psychopathic personalities and the feeble-minded. All we can say is that, considering patients diagnosed as "no mental disease," "psychopathic personality" and "manic depressive insanity" to be mentally nearer normal than the other psychotic patients,[5] and considering the "alcoholics" and "syphilitics" as nearer to normal than the "dementia praecox" and the "feeble-minded," then the highest per cent of judgments of "two" among the psychotic was attained by one member of the group next to the normal, and the next highest per cent by a member of the next group. However, when we arrange the subjects in such groups and compute the average performances by groups, no differences appear between the abnormal groups. Our number of cases is of course small for each diagnosis save that of feeble-mindedness and not too much reliance can be placed upon the results. It is rather surprising that the number of average normals reporting "two" is greater than the number of superior normals giving that judgment. On the face of them, these per cents seem to be against the general tendency of results, but there is a possible explanation of the difference. It will be remembered that we had a number of superior normals and one

[5] We do not claim that these statements of nearness to normality will hold true for the average of all cases of the different diseases, but merely that a physician who knows all the cases used in the Psychopathic Hospital and the writer, who tested most of these cases, agreed that the arrangement held good for this particular group of cases.

average normal who reported "more than two" and these we grouped with the "not two." If, on the contrary, we should group these with the "two" on the basis of discreteness of impression, we have 32 per cent of the superior normals and 33 per cent of the average normals reporting "two." We find, then, little difference between the two classes of normals.

Passing to the results classified by mental age, we find again a marked difference between the per cent of "two" judgments given by those of normal (though unmeasured) mental age and those grading at a low mental age. If we group our subjects as "normal adult intelligence" and "sub-normal intelligence" we find judgments of "two" given by 26 per cent of the former, but by only 2 per cent of the latter group. It is rather surprising that we have no marked changes in the per cent with advancing mental age, for we might expect that those having very low mental ages would be correspondingly unresponsive to tactual stimulation. It is true that, as we glance down the table, we find the 0 per cent predominating at the low mental ages, but the differences are so slight as to make it unsafe to draw hard and fast conclusions.

When the subjects are grouped by sex, we find no difference whatever in the responses. When the subjects are grouped by chronological age, we find again no systematic increase or decrease. The reason for the large per cent of "two" judgments at age 20 is not clear, unless it be due to the fact that there are many more cases at this age; and the chance that the per cents at the other ages would be raised if more cases had been tested.

In general, then, we find decided differences between the normal and the abnormal in reaction to tactual stimulation, and also a decided difference between normals and persons grading at a low mental age. No differences appear, however, when the subjects are grouped by sex or by chronological age. It seems possible, of course, that when they are grouped by sex and by chronological age the results are cut across by the intelligence factor so that any sex or age differences which are really there are concealed.

The results for the illusion test show that the superior nor-

mals are more apt to call the two horizontals "equal" than are any of the other groups of subjects.[6] Whether this result is due to superior mental ability or to the fact that twenty-four members of this group had 'seen' the illusion before, is not clear. Of those who had 'seen' the illusion before 86 per cent gave the judgment "equal." The average normals and the psychopathic personalities give the next largest per cent to the superior normals. An interesting point about this table is the distribution of cases reporting the left horizontal as the longer. Only one case among the normals and groups near normals gave such a reply. He is an average inhabitant of a country village who runs a small milk business, owning, perhaps, ten cows. His reply was, "Well, if you don't pay any attention to those cross-marks, you can see that this one is the longest." The other cases giving this response were eight feeble-minded girls (mental ages: 6.2, 7.7, 7.8, 8.1, 8.1, 8.2, 8.2, and 8.6) and one dementia praecox patient at Danvers who is reported as "much deteriorated." The probability is that these abnormal subjects were of such low grade mentally that any question concerning comparison of lines which were not placed one over the other would have been too difficult for them to answer. The normal subject evidently was perfectly capable of understanding the question, so that the above reasoning cannot serve for his case. It may be interesting to note that two superior normals suggested the possibility of the "left" being the longer. Both of these, however, were reflective, not immediate judgments.

Passing to the grouping by mental ages, we find here a decided increase in the per cent of "equal" answers with increasing mental age. In the first place the 48 per cent given by the normal subjects is far ahead of 24, the largest per cent given by any inferiors; and if we arrange the subjects in groups covering two years of mental ages, we find for ages 5.5 to 7.4 a per cent of 6.5, for 7.5 to 9.4, a per cent of 13.5,

[6] There is a possibility that our wording "Are the two halves equal?" suggests equality to the subject. The wording was chosen, however, with an effort to eliminate suggestion as far as possible. In our opinion it has succeeded to a great extent. At any rate, if it does suggest, there is no reason to suppose one group would be affected more than another.

for 9.5 to 11.4 a per cent of 14.0, and for 11.5 up, a per cent of 16.5; and then the jump to the normals at 48 per cent. Here again, as in the case of the first section of the table, we find answers of "left" appearing only at the low mental ages. In combining the two classes of normals, we have so many cases that the one case giving "left" does not appear in the table of per cents in round numbers.

When the subjects are grouped by sex, we find the males giving a larger number of "equal" responses than the females. The fact that 2 per cent of the females gave "left" while 0 per cent of the males gave that reply, is easily explained by the fact that the entire group of feeble-minded cases tested at Waverley were females, and this group may, in fact, influence the entire table. We had no similar group of feeble-minded males. It seems possible that this large group of feeble-minded girls is also influencing the part of this table which relates to the tactual experiment, thus concealing a possible greater sensitivity on the part of females in general. If this is true, it simply strengthens our conclusion that diagnosis, not sex, is the determining factor in our results, although in an investigation in which the numbers for the sexes were more nearly equal, a slight sex difference might still appear.

When the subjects are grouped by chronological age, we have again no regular increase or decrease in kind of answer with increasing age, except that there is a tendency in our subjects for ages under 20 to give the answer "equal" less often than do the older subjects. Perhaps the adults have learned by the age of 20, if they are ever to learn it, that "things are not always what they seem."

In general, then, in the illusion test we find, as we found in the tactual experiment, that the normal subjects give a larger per cent of correct replies (considering "equal" as more nearly correct than "right") than any of the groups of abnormal subjects. The difference once more holds good for the arrangement by mental ages. With the illusion, however, we find a tendency for females and for subjects under a chronological age of 20 to be more suggestible for inequality than other subjects.

We have now discussed the main arithmetical results

obtained from our investigation. There remain, however, a few points of interest in regard to unusual replies given in each of the tests. We find, for example, a number of subjects reporting sensations other than pressure for the tactual experiment. Twelve subjects reported temperature: 6 gave "warm" (1 average normal, 1 psychoneurosis, 1 psychopathic personality, 1 dementia praecox, 1 delirium tremens, and 1 insane epileptic); 3 gave "hot" (all feeble-minded); 1 (average normal) gave "cold;" 1 (average normal) gave "cool;" and 1 (neuro-syphilis) gave "burning." Four subjects (2 average normals, 1 psychopathic personality, and 1 dementia praecox) reported "electricity" or "electric shock."

The question of localization of pressure felt may also be of interest. No record was kept of the distance between the pressure given and its localization by the subject, though it was evident to the experimenters that as a rule localization was very inexact. The record which we did keep we have already described as a report of "one point," "two points," "line," "line across arm," "area," and "small area." The normals were the only class ever reporting more than two points felt. The varieties of localization arranged in order of frequency for all our subjects together are: "one point" (by far the most frequent, about 80 per cent); "line;" "two points;" "small area;" "area;" and "line across arm." The "line across arm" was never given except by the normals and two groups near normal, the "no mental disease" and the psychopathic personalities. The "line" and the two "areas" give no consistent results for the different diagnoses. We did not consider the data on localization (gathered merely to determine whether one or two points were felt) of sufficient accuracy to warrant any compilation of per cents by age, etc.

The question of the stimulus-error is one that might be expected to be of interest in our tactual problem; but it is clear that our instructions were such as to give the non-psychological subject a distinct bias toward that error. It will be remembered that we told the subject to "tell us what he felt." It is evident, at least in the light of our results, that the average person takes "what" to mean "what thing." It is consequently difficult to divide our answers into those show-

ing the stimulus-error and those not showing it, for the answers were in general so brief that the investigator must put his own interpretation upon them. For example: one subject answers "just a touch." The investigator considers this to be the untrained person's equivalent for "a pressure." The next subject reports "you touched me," and the investigator infers the stimulus-error. The probability is, however, that both subjects meant the same thing. Considering our results roughly we may say that superior normals tend to show the least stimulus-error,[7] and that the feeble-minded give a very large per cent stimulus-error. Although we can not state the amount of this error exactly, we can give some idea of the variety of names assigned to the stimulus. We find pin (51 times), pencil (43), finger (33), point (30), needle (17), paper (14), eraser (10), rubber (9), pen (9), wood (7), book (7), and electricity, cotton, hair, pasteboard, ball, block, bubble, dot, button, watch, spool, feather, something blowing on arm, cloth, chalk, wheel, and box, each one five times or less.

We have now discussed the immediate results of the tests used in our investigation, without reference to their theoretical bearing on the main problem: the practicability of the method for use with such populations as may be met with on anthropological expeditions.

The fact that we obtained results which varied when the subjects were grouped in some ways (such as by diagnosis) and showed no consistent variation for other groupings (such as chronological age) is an argument for the assumption that by various groupings we may discover which of a number of given variables we are measuring. In this respect the method is surely as reliable as that of giving a number of trials to every subject. The second point in favor of the method of single stimulation is the ease with which the data may be arranged for statistical study. Given an "all or none" method of scoring, the answers are readily arranged, and are easily interpreted by the average reader. The third, and, in our opinion, the greatest argument for the method is

[7] Possibly because a number of them (at least 23) had had some psychological training.

the ease with which tests are applied to a large number of subjects, and not limited to the selected group who enjoy being subjects for experimental investigations. It is, for example, ordinarily very difficult to persuade an average normal person to pass through a series of tests. He does not understand the point of view of collecting data, and takes the tests to be a means of showing some way in which he himself is "queer." If only one trial is used for each test, the subject actually does not have time to meditate on conclusions to be drawn, and his interest does not flag before he finds the tests are over. It is also possible to obtain replies from many subjects without telling them they are being tested. This is particularly true of tests like the illusion. One unsuspecting normal was, for example, shown the drawing with this remark, "Somebody showed me this the other day, and asked me if the two halves were equal. What do you think?" Moreover, it seems probable that the attitude taken by all subjects to the first trial is more uniform than the attitude taken toward succeeding trials. We should expect to find groups of subjects saying to themselves "Same thing right over again;" other groups saying "I suppose this will be different;" and others "Guess my first answer must have been wrong."

We conclude, therefore, that given field-conditions requiring simplicity of task, apparatus, and method, and involving large numbers of subjects, the method of one trial given to all available subjects is superior to a method necessitating several trials given to fewer subjects because:

1) the larger and more representative group of subjects may be tested;
2) no variable errors due to change of attitude toward the test can come in;
3) no errors due to practice can come in;
4) other errors may be assumed to cancel one another in the large group;
5) in spite of the simplicity of response, the data may be arranged to show the influence of different variables;
6) the results may be checked easily by results from other groups; and
7) the data are readily interpretable.

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